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The SCIENTIFIC MONTHLY

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NOVEMBER 1950

Meetings

Oct. 30–Nov. 3. American Public Health Association. Kiel Auditorium, St. Louis.

Oct. 31–Nov. 2. American Society of Agronomy (Annual). Cincinnati.

Nov. 3–4. Society of Rheology. Hotel New Yorker, New York.

Nov. 6–10. Gulf and Caribbean Fisheries Institute. Sans Souci Hotel, Miami Beach.

Nov. 6–10. Third Inter-American Congress on Brucellosis. Washington, D. C.

Nov. 13–15. National Fertilizer Association (Fall). Edgewater Gulf Hotel, Edgewater Park, Miss.

Nov. 13–16. American Petroleum Institute (Annual). Los Angeles.

Nov. 15–17. Industrial Hygiene Foundation of America, Inc. (Annual). Mellon Institute, Philadelphia.

Nov. 15–18. National Paint, Varnish, and Lacquer Association. Fairmont Hotel, San Francisco.

Nov. 24–25. American Physical Society. University of Chicago, Chicago.

Nov. 26–Dec. 1. American Society of Mechanical Engineers (Annual). Hotel Statler, New York.

Nov. 27–29. American Standards Association. Waldorf-Astoria Hotel, New York.

Nov. 27–29. American Pharmaceutical Manufacturers Assn. (Midyear). Waldorf-Astoria Hotel, New York.

Nov. 27–Dec. 2. 19th Exposition of Power and Mechanical Engineering. Grand Central Palace, New York.

Dec. 1–3. American Phytopathological Society (Annual). Hotel Peabody, Memphis.

Dec. 3–6. American Institute of Chemical Engineers (Annual). Columbus, Ohio.

Dec. 4–5. Chemical Specialties Manufacturing Association (Annual). Hotel New Yorker, New York.

Dec. 5–7. American Meteorological Society. Tallahassee, Fla.

Dec. 7–9. Southwest Section, ACS. San Antonio, Texas.

Dec. 13–16. Society of American Foresters (Annual). Mayflower Hotel, Washington.

Dec. 16–19. American Academy of Optometry (Annual). Drake Hotel, Chicago.

Dec. 26–30. American Association for the Advancement of Science (Annual). Hotel Statler, Cleveland.

Dec. 28–29. Division of Industrial and Engineering Chemistry, ACS. Baltimore.

Jan. 8–12. Society of Automotive Engineers (Annual). Hotel Book-Cadillac, Detroit.

Jan. 22–26. American Institute of Electrical Engineers (Winter). Hotel Statler, New York.

Jan. 29–Feb. 1. Institute of the Aeronautical Sciences (Annual). Astor Hotel, New York.

Mar. 5–7. Pittsburgh Conference on Analytical Chemistry and Applied Spectroscopy. William Penn Hotel, Pittsburgh.

Mar. 12–15. National Electrical Manufacturers Association. Edgewater Beach Hotel, Chicago.

Mar. 13–17. National Association of Corrosion Engineers (Annual). Statler Hotel, New York.

Mar. 26–29. American College Personnel Association. Stevens Hotel, Chicago.

Apr. 2–5. American Society of Mechanical Engineers. (Spring). Atlanta, Georgia.

Apr. 8–11. The Electrochemical Society. Wardman Park Hotel, Washington, D. C.

Apr. 9–13. American College of Physicians (Annual). St. Louis.

Apr. 16–18. American Society of Lubrication Engineers (Annual). Bellevue-Stratford Hotel, Philadelphia.

Apr. 16–18. Society of Automotive Engineers. Hotel Statler, New York.

Apr. 18–20. National Petroleum Association. Hotel Cleveland, Cleveland.

Apr. 23–24. Industrial Accident Prevention Associations (Annual). Royal York Hotel, Toronto.

Apr. 25–27. Natural Gasoline Association of America (Annual). Mayo Hotel, Tulsa.

Apr. 29–May 3. American Association for the Advancement of Science (Southwestern Division). El Paso.

Apr. 30–May 2. American Geophysical Union (Annual). National Academy of Sciences, Washington, D. C.

Apr. 30–May 2. Chamber of Commerce (Annual). Washington, D. C.

May 2–4. American Institute of Electrical Engineers. Syracuse, N. Y.

May 6–9. Liquefied Petroleum Gas Association (Annual). Stevens Hotel, Chicago.

May 7–10. National Fire Protection Association. Detroit.

May 12–16. National Fire Protection Association. Montreal.

May 13–16. American Institute of Chemical Engineers (Regional). Hotel Muehlbach, Kansas City, Mo.

May 14–16. American Gas Association. Hotel New Yorker, New York.

May 28–June 6. Third World Petroleum Congress. Kurhaus, Scheveningen, Holland.

June 11–15. American Society of Mechanical Engineers. (Semiannual). Toronto.

June 25–29. American Institute of Electrical Engineers (Summer). Royal York Hotel, Toronto.

Aug. 20. American Institute of Electrical Engineers. Portland, Oregon.

Sept. 2–8. American Chemical Society (75th Anniversary). New York.

Sept. 4–7. Pacific Coast Gas Association (Annual). Fairmont Hotel, San Francisco.

Sept. 8–9. XVIth Conference, International Union of Chemistry. New York.

Sept. 10–14. XIIth International Congress of Pure and Applied Chemistry. New York.

Sept. 11–20. Congress on Building Research. London.

Sept. 12–14. National Petroleum Association. Hotel Traymore, Atlantic City.

Sept. 15–16. XVIth Conference, International Union of Chemistry. Washington, D. C.

Sept. 16–19. American Institute of Chemical Engineers (Regional). French Lick, Ind.

Sept. 25–28. American Society of Mechanical Engineers (Fall). Minneapolis.

Sept. 26–28. National Metal Trades Association. Palmer House, Chicago.

THE
SCIENTIFIC MONTHLY

NOVEMBER 1950

Antarctic Mapping and Aerial Photography

FINN RONNE

Following the example of his adventurous father, who, at the age of sixty-eight, after more than twenty years of arctic exploration, accompanied the first Byrd Expedition, Finn Ronne has become a veteran polar explorer. He went on the second Byrd Expedition, and was with the U. S. Antarctic Service Expedition in 1939–41. After several years of active service in the Navy, Commander Ronne organized and led his own expedition to Antarctica in 1946–48. As the navigator, he flew in the trimetrogon-camera-equipped Beechcraft on all the long exploratory flights. Two women, one of whom was Mrs. Ronne, spent the winter with the expedition, an exploit unique in the annals of antarctic exploration.

ALTHOUGH Antarctica has been the goal for about one hundred and seventy expeditions from many nations for well over a century, two thirds of its 6 million square miles have yet to be seen by man. Even the coastal margins have not been fully delineated. Members of the Ronne Antarctic Research Expedition in 1947 flew over and photographed most of the least-known coast, a 500-mile strip of the Weddell Sea coastline.

Comprehensive mapping in the Antarctic was only one part of the work done on this expedition. Thorough studies were also made in geology, seismology, meteorology, solar radiation, and tidal observations, to mention just a few. The coordination of these and other data provides information of wide significance to geographers.

Early Mapping Methods

The methods used to map and chart the new land by the early explorers contrast sharply with those possible today. For some seventy-five years, until man first set foot on its shores, all mapping and charting of the continent of Antarctica were done from the decks of ships. Accurate sights of the sun and stars were difficult to obtain from the small sailing vessels, rolling and pitching in the violent antarctic seas, with sextants and chronometers, their main tools for determining positions, which lacked modern precision. Previously charted points for reference either did not exist or were of doubtful accuracy. Taking these things into consideration, it is with amazement and considerable admiration that we find how close they came at times to an accurate location of their newly discovered landmarks. Generally, however, an error of 50 miles was considered good navigation in those early days.

At the beginning of the twentieth century, both the north and south polar regions aroused in men an infectious desire to be the first to reach the poles, and serious exploration was often subordinated to this overpowering primary objective. Forced to travel over the surface with the bare minimum

Preparing planes for flight. Weasel in foreground and expedition ship, *The Port of Beaumont, Texas,* frozen in in the background.

weight-load, the early explorers usually left cumbersome transits behind. The magnetic compass, however, even with the problems it presented in proximal polar areas, was a boon to both ship navigation and surface travel. Mounted on a sled, the compass was used to take magnetic bearings to prominent features, and the easily manageable ship's sextant was relied on for navigation.

As man's tedious task of hauling sledges gave way to dog-power and mechanized surface transportation, and weight was no longer almost a life-and-death matter, the more accurate transit largely replaced the sextant, and the chronometer was supplemented by portable radio sets for receiving daily "time-ticks" sent out from the main base.

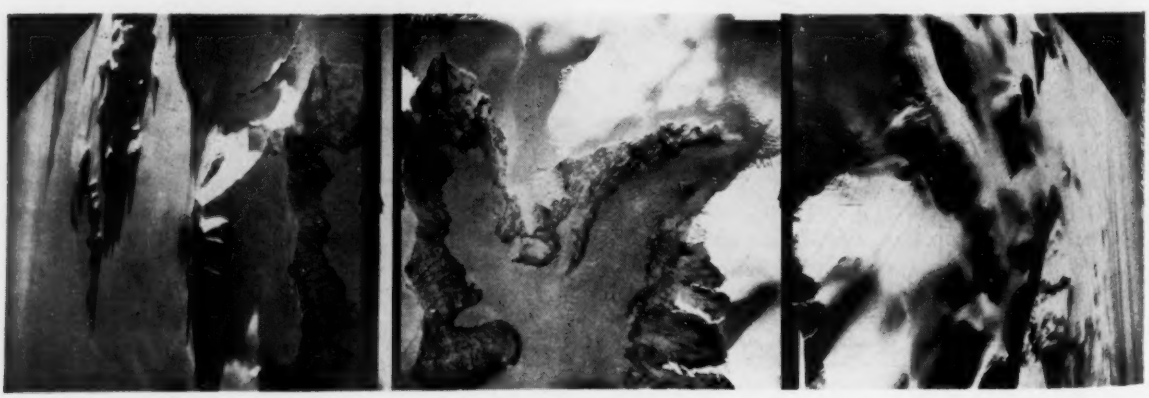
The innovation that dwarfed all others, however, as an aid to exploration was the airplane. With the development of aerial photography, the airplane opened up undreamed-of possibilities for the mapping of vast unknown areas. In the early stages of aerial photography and mapping, the limitations and inadequacies of oblique photography, and the near impossibility of reconstructing the plane's exact flight track on vacant grid-line charts, resulted in nearly as much inaccuracy in the plotting of topographic features as in the days before man first set foot on antarctic shores. These shortcomings were particularly evident when long flights were made without the benefit of ground control points.

Techniques Used on the Ronne 1947 Expedition

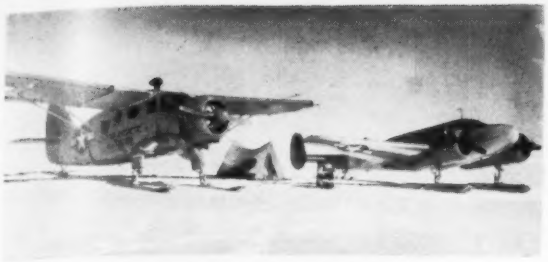
Aerial photographic equipment. Aerial photography and mapping were greatly accelerated during World War II, chiefly through the refinements in trimetrogon photography. By this method it became possible to photograph the entire area along the flight track from horizon to horizon, one camera taking a vertical picture of the terrain directly below, and the other two taking simultaneous oblique pictures on either side.

Such cameras were installed in one of the Ronne Expedition's airplanes, a twin-engine Beechcraft. An intervalometer was connected to the electrically operated cameras to expose pictures automatically, at a rate commensurate with the plane's speed and so controlled as to produce overlapping photographs, each of which could be placed in true relation to another. The latest navigational aids available—such as a radio altimeter, a view finder, and a drift meter—were also installed.

Network of weather stations. We recognized in the earliest planning stages of our expedition that accomplishment of the aerial photographic objectives would depend in large part on the weather; a network of stations was therefore planned at strategic locations in the general area of intended operations, to report weather observations to the meteorologist at the main base for correlation and forecasts before flights. The weather forecasts served three primary purposes: safety, gasoline conservation, and continuity of photographs. Knowl-



The three portions of a trimetrogon photograph. Center picture is vertical with left and right obliques along the flight track. Overlapping features can be noted along adjacent edges.



Cape Keeler weather station.

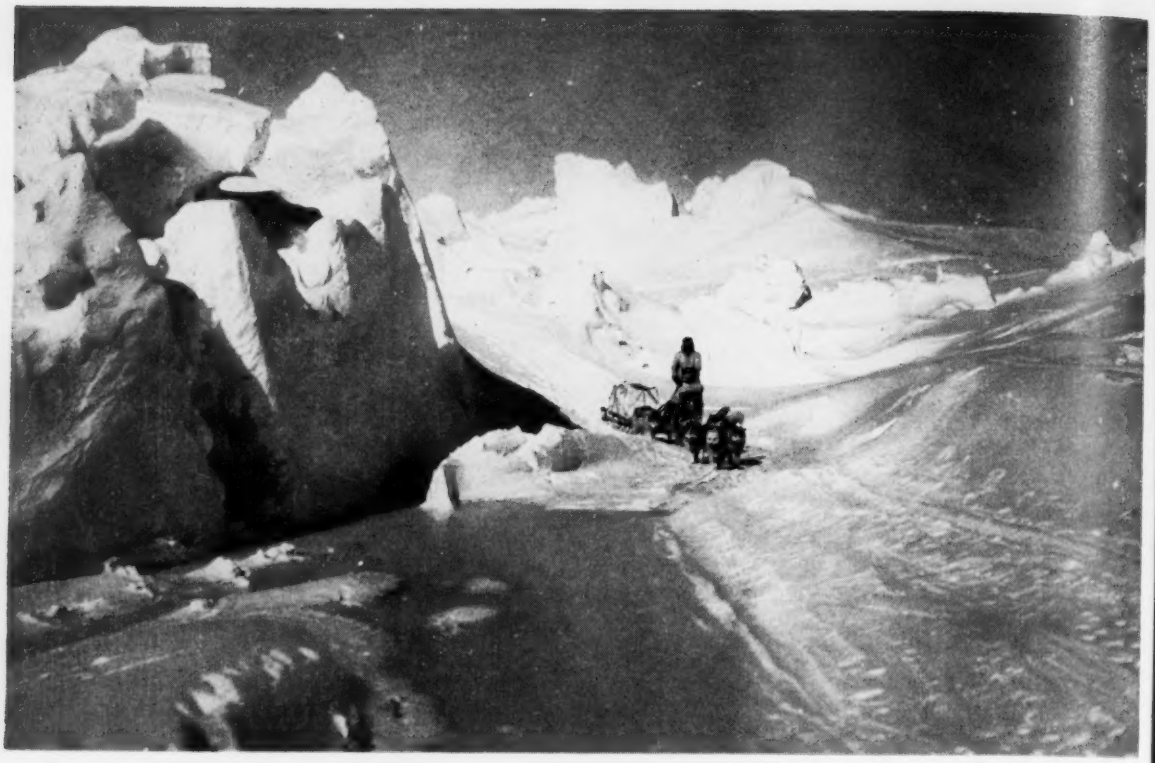
edge of the weather conditions along the flight track was an essential safety precaution, for if the weather should close in suddenly during a flight, the pilot would not have much chance to make a safe landing among the mountains and glaciers of the Palmer Peninsula; aviation gasoline was so important a limiting factor that to ensure its not being wasted it behooved us to know whether a flight could be completed, even before the heaters were applied to the airplane motors. And the cloud cover within the area of a photographic mission had to be known to provide identifiable continuity in the resulting photographs.

Early in the antarctic spring of 1947, the plan was put into operation by establishing a meteorological station on the Palmer Peninsula plateau 17 miles east of the wintering base; it was manned for 104 days. A second meteorological station was operated continuously for 74 days at Cape Keeler, 125 miles southeast of the base on the opposite side of the peninsula. Supplementary weather information was relayed to the main base from dog-team parties working in other areas. It can now be stated that if such a network of weather stations had not been established our aerial photography and mapping mission would have failed.

Most of the photographic missions were flown at an altitude of 10,000 feet, although altitudes of 14,000 feet were necessary in some of the mountainous areas. To assist in the correlation of the photographs, the camera operator maintained a constant check on changes in altitude, and the navigator obtained lines of position from sun sights with a bubble sextant. Careful records of driftmeter readings aided in determination of the flight track and the ground speed. The intervalometer was set at appropriate intervals, depending on the altitude and ground speed, so that each photograph would overlap the preceding one by 60 per cent.

These procedures were quite effective for the more general survey work, but, by using two planes in what might be called the "leap-frog" method, we were able to locate more precisely the major geographical features. The two planes, one a single-engine Norseman carrying gasoline, and the other, a trimetrogon-equipped Beechcraft, would proceed southward from the main base together. At a predetermined distance from the base, the planes landed in the field to transfer gasoline from the Norseman to the Beechcraft, thereby extending the latter's flight range. While on the ground, we obtained accurate positions from celestial observations, and bearings were taken to the most prominent features in the surrounding landscape. Numerous landings were made in the field, some predetermined and others necessitated by sudden changes in weather conditions.

Gasoline transfer.



Dogteam party going through Pressure R.

In addition to fixes obtained by landing airplanes in the field, other surface observations were gathered by a fast-traveling sledge party of four men and twenty-seven dogs, supported by air. The party sledged for 106 days and carried out detailed survey work for a distance of 200 miles, operating also as a weather-observing unit and stand-by party for the long exploratory flights into the unknown.

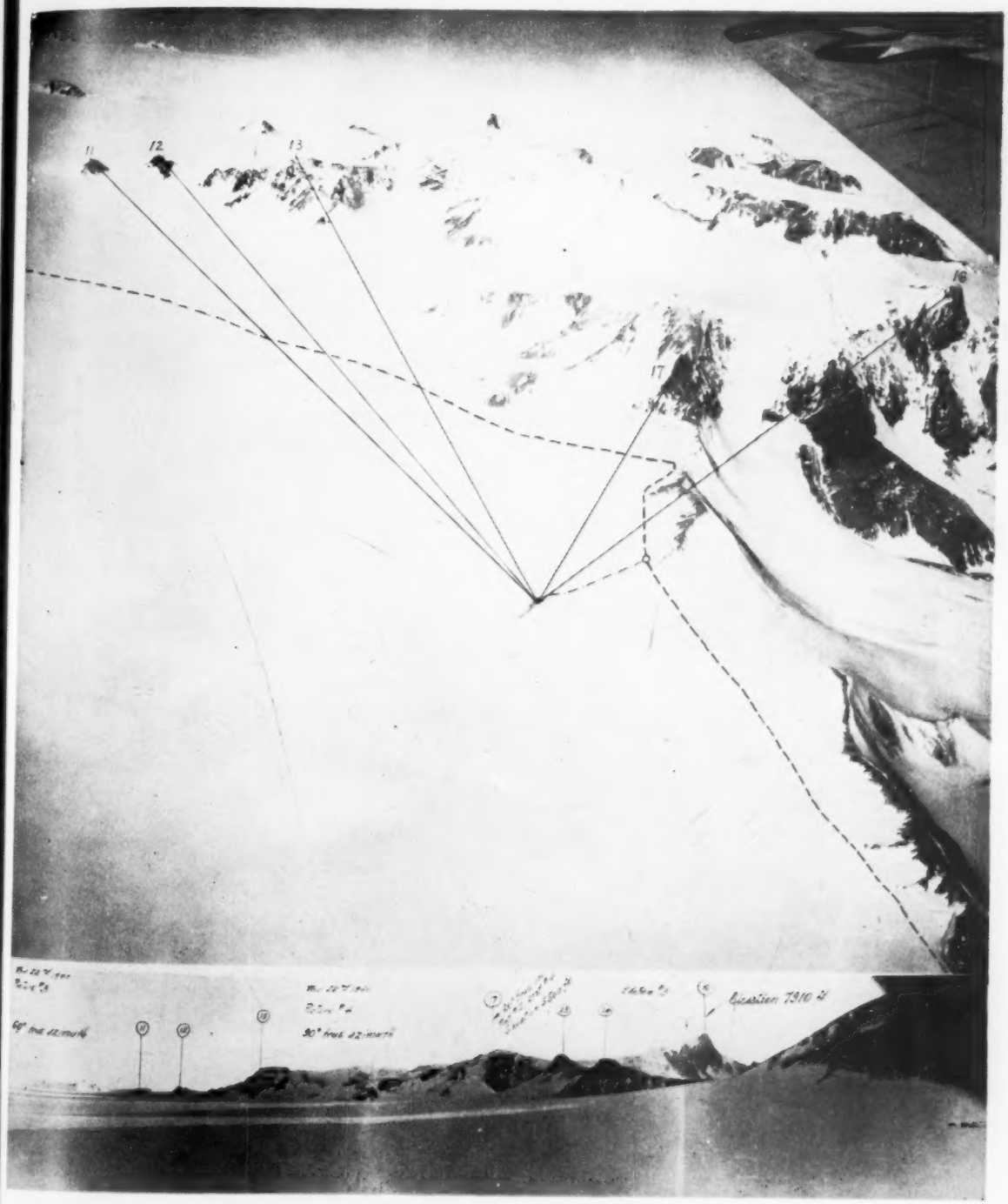
The network of ground control points obtained from both the leap-frog method and from the surface sledge party was costly in gasoline and man-hours, not to mention the anxiety that accompanies flights and sledge trips into unknown territory hundreds of miles from the nearest base. Justification for these expenditures is really very simple, however, when one considers that, unless the latitude and longitude of the significant topographic features on aerial photographs are known, the pictures are next to useless for mapping.

Difficulties encountered. The antarctic summer is the only feasible season for making photographic flights. Aerial photographs taken during the rest of the year under poor light conditions are of little value. Without the sun, the pictures will show no shadow and hence will not exhibit the sharpness and clarity so essential for picking out details in later correlation and cartography. Polar flying also encounters a serious hazard when the sun is below the horizon or obscured, in the appearance of the white surface as one solid mass of nothingness, with no contrast from which to determine a good landing surface.

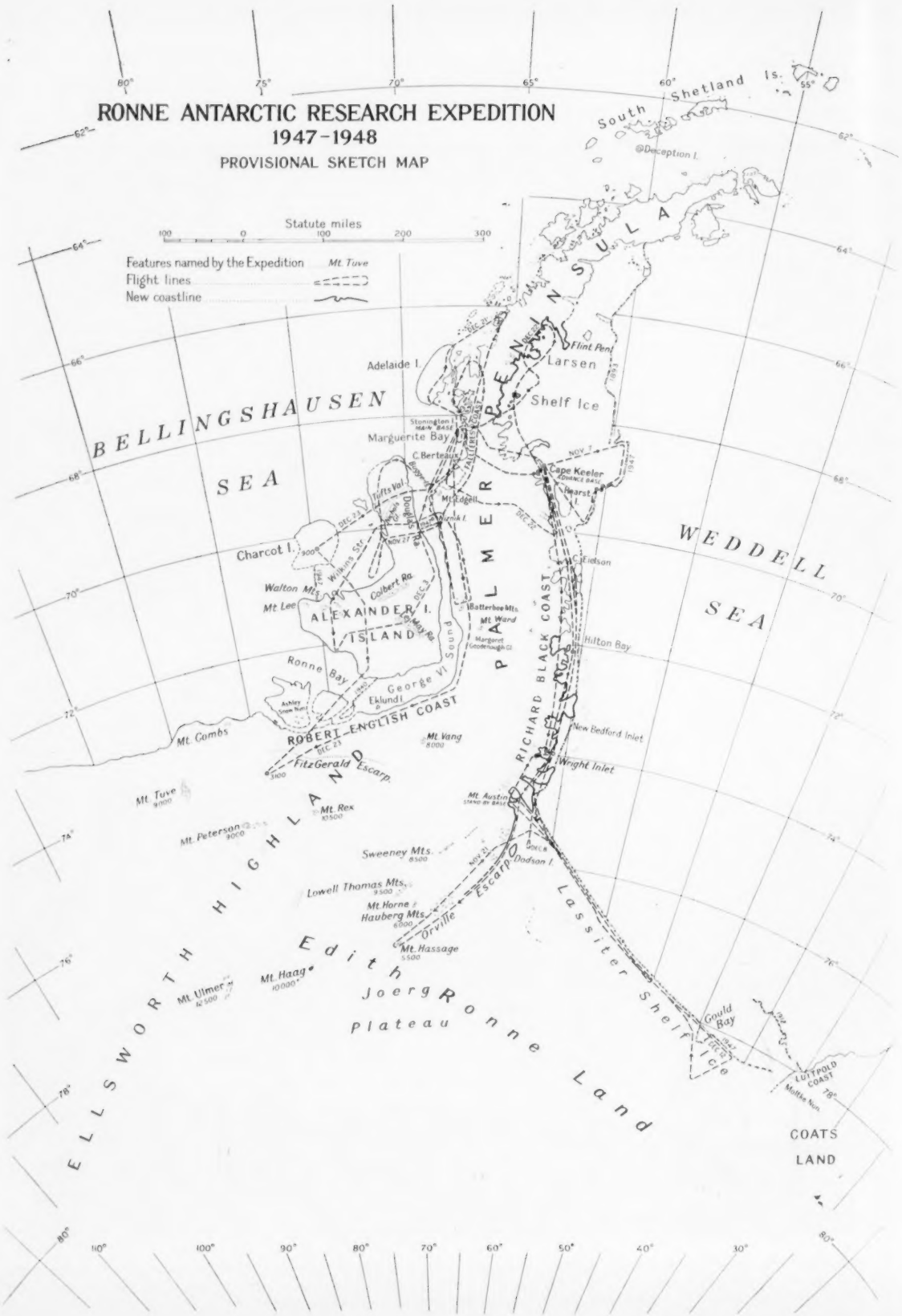
The flying season in the Palmer Peninsula generally lasts from November to early February, but, since this is also the only period when ice conditions along the coast permit a vessel to reach the mainland, it was necessary to establish a base some time in February or March for occupancy during the winter, to utilize fully the flying season of the following summer. Even so, there were few really ideal flying days during the 1947–48 season, and what little good weather we had lasted for only a few hours at a time. Few flights lasted more than 7 hours, although the total of 346 hours in the air compares favorably with air operations of other antarctic expeditions.

Unfavorable cloud conditions were not the only difficulties caused by weather. Although the flights were made during the summer months, low temperatures at flight altitudes caused malfunctioning in the trimetrogon camera mechanisms, in most cases because of congealing of lubrication and unsuitable design features in delicate units, such as bearings. For example, pivots snapped in the driv-

ing gears of the cameras. On some flights this malfunctioning was not discovered until after the plane had returned to base, for the camera indicated the film had wound normally on the spool. The cold also led to an overload of the electric circuits on one occasion, during the first flight into unexplored land south of the Weddell Sea. Camera heaters had to be used, and when the radio altimeter was operated the safety fuses blew out. When only one fuse remained, the altimeter had to be turned off.



Combination air and ground picture. Dotted line on aerial-trimetrogon photograph shows route of a sledge party. A fix was obtained at the point from which the radiating lines are drawn to specific mountain features. Same features are shown in lower photograph, which was taken on the ground by the sledge party.


RONNE ANTARCTIC RESEARCH EXPEDITION
1947–1948
PROVISIONAL SKETCH MAP
Statute miles
100 0 100 200 300
Features named by the Expedition Mt. Tuve
Flight lines
New coastline
South Shetland Is.
Deception I.
Flint Peninsula
Larsen Shelf Ice
Adelaide I.
BELLINGSHAUSEN SEA
Marguerite Bay
Stonington I.
MAIN BASE
Base E
C. Berteaux
Cape Keeler
Base F
Fossil Bluff
Base W
Mt. Liddell
Arnell I.
Tuffs Val.
Charcot I.
Wilkins Sea
Walton Mts.
Mt. Lee
Colbert Ra.
ALEXANDER I. ISLAND
Batterbee Mts.
Mt. Ward
Margaret Goodenough Gl.
PALMER
Ronne Bay
George VI Sound
Eklund I.
Ashley Snow Num.
ROBERT ENGLISH COAST
Mt. Combs
Mt. Vang
8000
FitzGerald Escarp.
3100
RICHARD BLACK COAST
Hilton Bay
New Bedford Inlet
Wright Inlet
WEDDELL SEA
Mt. Tuve
9000
Mt. Rex
10500
Mt. Peterson
9000
Mt. Austin
Base B
Dodson I.
Sweeney Mts.
8500
Lowell Thomas Mts.
9500
Mt. Horne
Hauberg Mts.
6000
Orville Escarp.
Mt. Hassage
5500
ELLSWORTH HIGHLAND
Edith Joerg plateau
Ronne Land
Lassiter Shelf Ice
Gould Bay
Mt. Ulmer
12500
Mt. Haag
10000
LUITPOLD COAST
Moltke Nun
COATS LAND

Contributions to the Antarctic Map

The Ronne Antarctic Research Expedition made what has been called the world's last remaining major geographical discovery, in establishing that Antarctica is one continent and not two. In 346 hours of flying the planes covered more than 700,000 square miles, 250,000 of which were newly discovered land. More than 14,000 trimetrogon aerial photographs were taken, which will be correlated with the astronomical fixes and ground control points to make new maps of the entire area covered.

New Techniques and Instruments for Future Antarctic Use

Future expeditions to the Antarctic will have available a wealth of new devices that will permit an even better job of mapping the remaining unexplored 4 million square miles to be done. Carefully drawn plans for use of such devices will have to be made well in advance of an expedition's departure, and coordinated with cartographic practices used in this work. Electronic equipment will play a large part. Shoran undoubtedly will contribute greatly to further mapping of the Antarctic. After several Shoran stations have been established, ground control points will be fixed and recorded automatically at the instant a set of photographs is taken. Radar photography will also be used in conjunction with trimetrogon photography to extend horizontal ground control between fixed positions, thereby providing continuous coverage even though clouds interrupt visibility. For vertical control, airborne radar altimeters will be synchronized with the trimetrogon cameras to obtain a continuous profile of the ground surface. These and other new developments, had they been available to us at the time of our expedition's departure from the United States in January 1947, would have made the laborious task of obtaining ground control points much easier and would have enabled us to cover an area several times larger.

But who can afford the outfitting of such an elaborately equipped expedition? In my opinion, the era of the private expedition to the Antarctic has passed, and only the government can cover the necessary expenditures. In times such as these, when technological developments are making such great forward strides, it would seem only reasonable for the three divisions of the Department of Defense—Navy, Army, and Air Force—to pool their excellent resources and undertake a comprehensive mapping program in the Antarctic. Comparatively modest expenditures today may give a good return in the future, for each of the 4 million square miles of unexplored territory must be assumed to have potential value at some time, if not now.

Taking sunsight on ground.

Some Psychological Undercurrents of Scientific and Medical Writing*

ALFRED PLAUT

Dr. Plaut, who recently became Chief of Laboratory Service at Winter Veterans Administration Hospital, Topeka, Kansas, was born in Frankfurt-am-Main and educated in Germany and France. He came to this country in 1922 and has since taught at Cornell University Medical College and at New York University Medical College. He specialized in tissue pathology. *(See page 337 for a discussion of some other aspects of the problem of language.)*

THE almost self-explanatory word "autistic," which has become an integral part of psychological vocabulary, was coined by the Swiss psychiatrist Bleuler,[1] in 1912, in a paper entitled "The Autistic Thinking." In a later publication,[2] Bleuler defined the term autistic as

> based upon the primeval soil of wishes and imaginations instead of upon reality and strict logic conclusions. [There are, according to Blueler] all transitions, belonging to one series with essentially qualitative differences only, starting with the dreams of the boy who plays the general on his hobby horse, through the poet who, in a piece of art, deals with an unhappy love and transforms it into a happy one, down to the hysteric and the schizophrenic who, in his hallucinations, sees his most impossible dreams fulfilled.

In a previous paper,[3] I attempted to show how the autistic attitude has influenced and still does influence our scientific theories, and even the practice of medicine. It became evident during this study that our modes of expression in science and medicine, our writing and talking, are at least as often autistic as the contents of scientific thinking are. No scientist will be at a loss to find examples of autistic language in his special field. Because I am a pathologist, my examples come mainly from medicine and pathology.

Why do we have so much jargon in medicine or "gobbledygook"[4] in science? One answer is: Because we are autistic. One of the mainsprings of scientific jargon is closely connected to autism; it is self-aggrandizement, self-assertion, or megalomania, whichever term we may like best or dislike least. Our forefathers in medicine and science spoke Latin, the vernacular was below their dignity. Many of us still seem to feel that plain English is below our dignity. It sometimes is hard to explain the needless deviation from ordinary language by anything but the unconscious wish to be different from the crowd, to be something better. Why does a man write "both upper maxillae are edentulous" when he means "the upper teeth are missing"? What is the advantage of "hemorrhaging" over bleeding? When we read "the carotid artery is seen to be considerably reduced in size," nothing more is conveyed to us but that "the carotid artery is thin;" and, similarly, "there is no evidence of gastric distention" means "the stomach is of normal width." Or, finally, "examination of a specimen of urine revealed positive evidence of the presence of tubercle bacilli, but the patient showed a much improved picture" might be condensed into "tubercle bacilli were found in the urine, but the patient looked much better." More examples could easily be found in the writings of any specialty. The plain statement is despised. It must be made more complicated, more sophisticated, supposedly embellished. Words like "the presence of," "the formation of," "replacement by," etc., often add nothing to the statement of fact. Talking in abstract terms is considered a badge of the highly educated. The misuse of abstract words has become ludicrous. "There is slight wetness of the skin" instead of "the skin is moist" strikes us as funny, but we are accustomed to "the serology is negative" or "there is no pathology in the brain." Soon we shall read, "There is no psychiatry in the patient."

In scientific writing, as elsewhere, such signs of self-assertion, of hidden arrogance, are mingled

* Sponsored by the Veterans Administration and published with the approval of the Chief Medical Director. The statements and conclusions published by the author are a result of his own study and do not necessarily reflect the opinion or policy of the Veterans Administration.

with symptoms of insecurity. Many redundant, meaningless words point to insecurity. A physician describes something as being situated on the upper surface of the liver, but he writes "in the region of the upper surface," etc.," and instead of "there is" we read "there seems to be." True inner freedom, freedom from the desire to be more than one is, and freedom from unjustified doubt in one's competency will show itself in clear, concise writing, in calling things by their names, without hedging.

But why are we so easily pushed off the straight road of simple, honest description? Because we lack the ability of seeing, which, according to Hippocrates, constitutes a large part of the medical art. Goethe once stated in a distich that what people consider the easiest thing is in reality the most difficult, namely, "to see with one's eyes what is spread under one's eyes." This deficiency in the art of seeing expresses itself in the vague terms of our descriptions and in the preference for abstract words. We often do not see what is before our eyes, but what we expect to be there. Terms like "hypertrophy," "edema," "cellular," etc., do not denote visual impressions, but conclusions drawn from various premises. Such conclusions may be premature or wrong. In autistic preoccupation with ourselves, we do not see the objects we are studying, as such—instead we see them a priori combined with our ideas about the objects. In the word's literal meaning, we do not see *objectively.* This is shown in our choice of words and phrases. We mingle actual observation and conclusions. The techniques that furnish us with graphs and figures, like electroencephalography, electrocardiography, and blood chemistry, have dulled the senses and have decreased efforts at direct observation. More or less unconsciously, physicians wait for the reports from the biochemist, the roentgenologist, etc., and do not go to the trouble of using their eyes, ears, and fingertips sufficiently. Auscultation, percussion, careful inspection, and palpation have yielded more ground to the roentgen rays and to laboratory methods than is good for sick human beings and for unsolved problems.

We are deficient not only in seeing what is before our eyes, but also in seeing with the spiritual eye. Our writing is flat and unconvincing because we use words without seeing the corresponding object before the inner eye while we write; thus we do not convey a vivid picture to the reader. Vague adjectives are used—"prominent" or "conspicuous" when the meaning is large, high, thick, or deep, in the case of objects, or loud, sharp, rough, or prolonged in the description of a sound. A stomach ulcer is described as "prominent." Shall we, when reading this adjective, see before our eyes a large, shallow defect, or a small, deep one?

Descriptions made without linking the word to the object do not evoke the image of the object in the mind of the reader, or they create a wrong image. The word "cross section," according to horse sense and according to Webster, means a cut at right angle to the longest diameter. It often is applied to lengthwise sections as well, which sometimes is entirely misleading. The blame for this does not fall on medical people and scientists alone. *The New Yorker* could not carry its amusing standard item, "Block that Metaphor," if most people were in the habit of seeing, of visualizing while they write. And the same thing happens in today's poetical writing. The verbal world of the books threatens to conquer the dimensional world of our senses.

The English language offers ample choice of synonyms, but little use is made of them for enlivening scientific style. The truly convincing writer is the poet. He makes the "primeval words arise behind the ordinary words." The poet is a "seer;" he *sees,* not only with retina, deep brain centers, and visual cortex, but with association tracts and frontal lobe; he can reverse the process through his motor spheres, and by his writing make other people see *what* he has seen and *how* he has seen it. The scientist should strive to do the same.

Fortunately, I am not alone in such ideas. We read in the *Bulletin of the Menninger Clinic,* May 1947:

> Psychiatry, above all other branches of medicine, requires skill in verbalization. Things must be explained to patients and to patients' relatives and to other physicians. . . . That psychiatrists are so often accused of talking mumbo-jumbo is not so much because psychiatrists know so many technical words as because they are unskilled in the use of simple, explicit English.

Polytechnic institutes (Rensselaer) and departments of chemistry (the University of Texas, e.g.) have introduced programs to help students think and write clearly.

The problem of poor writing is insolubly coupled with the problem of poor thinking. Thinking and wording cannot be dealt with separately; they make each other.

At this point a larger problem must be considered, namely, the relationship between language and culture, language in general, and today's Anglo-American language in particular.

One characteristic of today's language, scientific and otherwise, is what we might call the flattening out, the leveling. Differences in meaning are neglected, one term is considered as good as the other.

Meaningful words are used on so many inappropriate occasions that their distinctive meaning is lost. The phrase "to visualize something" always appealed to me as a beautiful one, meaning to form a mental image of something that is not visible to the bodily eye, be it the beloved face of an absent friend, or a cherished utopian idea. But technicians have got hold of the word, applying it in the, originally rare, sense of making something visible; and the beautiful word is ruined. The verb "to create" and the adjective "creative," for example, two words pregnant with meaning and beauty, have been misused into insignificance in psychology and education, as well as in advertising. The word "research" has been equally mistreated. When people look for something in catalogues and in reference books they call it research, putting it on the same level with attempts at exploring the atom or the human soul. Two little gadgets put into a low-priced motor car make it "De Luxe;" add two more and it is "Super De Luxe." Terms referring to institutions of education are used so indiscriminately that the words "university" and "college" have lost their meaning. We let children "graduate" from kindergarten. It is a bitter joke that someone calls his training school for dogs a dog college, giving out the degrees of A.B. (always behaves), M.A. (masterful animal), and Ph.D. (phenomenal dog).

Such indiscriminate use of words, which comes from autistic attempts at aggrandizement and from laxity in logical distinction, in turn, dulls the sense for logical distinction. Some people have recognized the importance of this vicious circle. Christopher Morley is one of them. He writes, in *The Man Who Made Friends with Himself:* "My life was difficult because I loved words. I perceived their terrible power; I tried not to modify their meanings. Their overproduction and misuse are the stigmata of our perishing culture."

No one can—and who should?—try to stem the constant flux of language. But who directs the stream for us today? The advertising pages of the newspapers and magazines, the sport writers, the Broadway shows, the funny papers. There is justifiable cause for alarm when a recent serious and well-received book about the art of writing warns against too much reverence for grammar and punctuation, and cites the magazine *Time* as an example of "good, readable nonfiction writing."

Who neglects language neglects logic. Language is not merely a means of designating objects, it is a strong vital force, doing good and working evil. It is a candid-camera picture of our mind. As great a scientist as Buffon said, "The style is the man." Careful attention to language is an endeavor worthy of every scientist.

The pitfalls in writing, scientific or otherwise, vary with different languages. English, more than other Indo-Germanic languages, has lost most of its grammatical complexity, to the joy of teachers and pupils. But, with that, much of the distinctness, power, and beauty of our language has disappeared. When the forms of words become more and more like each other, character and beauty are more difficult to achieve in writing. A highly inflected language, like old Greek, has special forms of words for the concept "two" and thus can describe the bliss of matrimony with a poignancy unattainable in any modern language.[5] On the other hand, the English sentence, "The teacher traveled with a friend," does not indicate whether two men traveled together, or two women, or a man and a woman. Since this loss of grammatical complexity has made English less descriptive than Romance languages, for example, it becomes even more imperative for scientists to write vividly, with seeing eyes.

An intimate relationship between a man and the language he speaks and writes is more difficult to establish in English than in other languages. Only in English does the sound of a word so often fail to indicate its spelling, and vice versa. In learning to read and write, therefore, our children must accept the word as a Gestalt, as an entity. We thus do not develop the habit of analyzing words, we do not feel that they are composed of parts, of roots, and accessories. The origin of words, the development of their meaning, cannot be as evident to English-speaking people as to the Italian or the Frenchman, for instance. We therefore are less conscious of the original meaning of words and more apt to misapply them.

The close kinship between logic and grammar was recognized centuries ago. It was, for a long time, an undisputed creed of educators. Widely forgotten in the past two generations, it is in the process of being rediscovered. Philologists, semanticists, and scientists are beginning to pay attention to the close relation between grammar and syntax on the one hand, and logic on the other. Grammatical terms like active and passive, temporal and conditional, reality and unreality, statement and wish, and the rules governing their expression in language initiate the young student automatically into the principles of logical thinking. This was one of the great factors in the old-fashioned humanistic school system that started young children on Latin. This system, with its accent on dead languages and on knowledge of antiquity, was a heritage from the

times of the Renaissance, when mankind pulled out of the Middle Ages, when people stopped thinking only of eternal life and their chance of salvation, when they returned to human values connected with their life on this earth and with their fellowmen.[6] They found a source for such values in the rediscovered Greco-Roman culture, in its statuary, its poetry, its philosophy, its rhetoric, and its law. We, too, are in need of new values, if we want to pull out of the mire that threatens to engulf us, the mire whose name is technology.

Together with the knowledge of the classics, there came, in the Renaissance, good writing in the vernacular, notably in Italian. A feeling for balance began to prevail in all phases of human endeavor, an awareness of what is great and beautiful. And at the same time science began to flourish.

The foregoing might seem to have led us away from the topic of scientific style and its psychological undercurrents; but this is not the case. The autistic attitude, whose bad influence upon scientific writing has been discussed, could be counteracted if we had, in our educational system, something to take the place of the abandoned humanistic studies. We cannot hope for a high level in scientific writing unless scientists are given a well-grounded liberal education.

A fraternity address delivered recently at a Southern college was entitled "Ancient Answers to Today's Curriculum Problems."[7] In it we read:

> It is a matter of simple historical fact that we of America possess a culture rooted in the soil of the distant past; and that this soil is Judaism, Christianity and Greco-Roman civilization. Without an understanding of our continuing indebtedness to these ancient institutions, any real appreciation of American and of Western culture is impossible. [It says further] I do not in any sense pretend to assert that the classical tradition . . . the study of classical languages and civilization will alone realize the objectives of education. But I do maintain that in these studies there is a core, a basis, a radiant center . . . intelligent and consistent attention to which will aid, as few other studies can, towards reaching . . . high achievements.

This is not the place for a dissertation on humanistic education. A few points, however, must be mentioned because they bear on the psychological basis of a man's style. A teen-ager when studying Greek culture, art, history, or drama is confronted with the great principles of human endeavor, of human knowledge and human suffering per se, in an abstract way, not related to the history and the pride of his nation or his church. Virtues like fortitude or self-sacrifice as such are impressed upon the young mind. A basis of conceptual thinking is thus laid, without mentioning the term. The very same remoteness of the teaching matter that has drawn the scorn and the mockery of educators upon the old-fashioned humanistic teaching methods is an important aid to clear thinking and clear writing. The main tenets of Greek classic wisdom—moderation, and the harmonious, balanced personality—will enable a scientist to forego attempts at self-satisfaction that would mar his scientific style; it will keep him from writing autistic jargon. It will help him to discern between facts on the one hand and his own more or less subconscious desires on the other.

Study of languages shows us that things can be looked at, and are looked at, in different ways, not only in the way to which we are accustomed. It helps us in overcoming narrow self-righteousness. The differences in expressions of endearment, of politeness, of accusation, in designations of kinship, of custom, and law, all tell us that our concepts, outlined as they are by the vocabulary and the syntax of one language, our mother tongue, are not the only possible ones and not necessarily the best ones. In the same way we will realize that our scientific theories may not be the only ones and best ones, and this cannot fail to improve our writing. We thus can learn the kind of humility that is conducive to honest, scientific writing. Such humbleness, such freedom from self-assertion, is possible only in those who have their share of inner security. I have little doubt that familiarity with the great achievements of the human spirit, especially when acquired in the impressionable years of early puberty, are a better source of such security than any amount of high-school or college cramming in our own scientific fields.

These remarks, sketchy as they are, may indicate that the problem of scientific writing has important psychological aspects. For remedy, we might look more to the high-school teacher than to the university professor.

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Determining Geologic Age from Radioactivity

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HOW old is the earth? How old are certain deposits, certain formations, or certain minerals? Early estimates were made by whatever means were available at the time, such as the salt content of the oceans, the cooling of the earth, or the measurement of strata of deposition with fossils for the correlation of sections. This led to determination of ages of 100 million to 300 million years. These estimates were at best incomplete and rested on insecure foundations, necessitating drastic revisions from time to time.

Today, through chemistry and physics, a more elegant and a more powerful method of measuring geologic time has been developed, and the older figures have been multiplied at least tenfold. Ages of such staggering immensity were obtained that the very geologists who had not been satisfied with lower figures were unable to believe them. These new estimates, however, were gradually accepted and by the middle of the 1930s became common knowledge. Geologists helped to make them so generally known through magazine and newspaper articles that the layman is now perhaps too well adjusted to ages reaching into the billions.

A New Branch of Chemistry and Physics

How are these ages obtained, and how accurate are the methods? How is radioactivity associated with age determinations?

Radioactivity is a comparatively new branch of chemistry and physics. Just before 1900, the cornerstones of this modern branch of science were laid by the discovery one after another of X-rays and the elements polonium and radium. Soon after the discovery of radium by the Curies, Strutt and Rutherford made a very careful examination of some of the seemingly strange properties of these radioactive substances. The first startling conclusion was that radioactivity represented the disintegration of the atom forming new elements with the liberation of energy and certain rays whose characteristic properties were unknown at that time. It was found that one group of the particles given out by uranium was actually helium atoms with positive charges (now known as alpha particles). As soon as they regained their electrons these were found to be neutral helium atoms. Soon after the rate at which helium is generated by uranium was determined, Rutherford and Strutt in 1904 suggested that, if by any chance all helium being formed by the uranium were trapped in a mineral, then the amount of accumulated helium formed by the radioactive process would give an accurate indication of the age of the mineral.

Many minerals were analyzed, a number of which were of known geologic age: some recent, some intermediate, and some the oldest known. At the same time minerals containing no radioactive element were carefully examined. Startling results were obtained. Helium was found in all radioactive minerals—much more in the older than in the younger ones. The helium age corresponded to the geologic age. For the oldest minerals an age of about 700 million years was indicated, a figure approximately six times the then accepted maximum age of the earth itself.

The helium method has always presented many difficult practical problems. It was realized from the beginning that the figures obtained by this method represented minimum age values. Helium being a gas, and a light gas at that, leaks from even the most tenacious minerals. In the 1930s the helium method was subjected to intensive research, and it was found that under the most favorable conditions only 50 per cent of the helium was retained by the mineral. It is notable that, under such circumstances, results of any value at all have been obtained.

The year 1905 marked the development of two

remarkable fundamental theories in radioactivity. Einstein formulated his now well-known equation E equals mc^2, showing the equivalence of matter and energy, and also predicting the liberation of tremendous amounts of energy in the conversion of even a small amount of mass. In the same year, Boltwood first suggested that lead is the end product of uranium disintegration, the basic principle upon which our best radioactive age determinations are based. He came to this conclusion from the examination of the analyses of a number of uranium minerals, each one found to contain lead, even though the uranium mineral was not associated with lead minerals. Holmes and Lawson were later active in developing this idea and adding to the evidence that lead is found in uranium minerals.

Two years later, Boltwood strengthened the foundations upon which the lead method is based by examining minerals of different ages and finding that minerals supposed to be of the same age had identical lead-uranium ratios; that is to say, there is a constant proportion between the amounts of the disintegration product—lead—and the parent substance, uranium. At that time it was not suspected that thorium might also change to lead; its end product was thought to be bismuth.

Boltwood went on to point out that, if the amount of radioactive lead occurring with its parent substance uranium and the rate at which uranium disintegrates to form this lead were known, the length of time required to produce the lead could be calculated, and thus the age of the mineral and the geologic age (in terms of years) determined. From the analysis of various minerals, Boltwood determined geologic ages up to 2,200 million years. At the time, his figures were not generally accepted, for they were too great even for the geologist. It was not until 1914 that Boltwood's results were confirmed. He had also made the prediction that the end product—lead—would have an atomic weight of 206. If uranium disintegrates, forming helium, which has a weight of 4, then the loss of 8 helium atoms, totaling 32, from the weight of uranium as 238, leaves 206. Ordinary lead was known to have an atomic weight of 207.2. T. W. Richards, of Harvard, in 1914 made a careful atomic weight determination on lead obtained from a uranium mineral and found its atomic weight lower than 207 and close to 206. This put the theory and actual determinations of age on a firmer basis. It also astonished the chemists, for never before had the atomic weight of an element been found to differ. Before this, atomic weight was considered a constant property of each type of matter.

From time to time it has been suggested that lead may not be the real end point of the radioactive series. So far no conclusive evidence has been produced to prove that it is not. Even if it is very slightly unstable in the uranium series, it is probably so slow as not to affect our calculations of age.

Other factors that could affect the rate of disintegration of radioactive elements, such as change in temperature, physical state, pressure, or some form of chemical combination, have all been carefully investigated and found to have no effect. Temperatures from that of liquid helium to the temperature of the electric arc have shown no change in the constant rate of radioactive disintegration.

Thorium and actinium are now also known to produce radiogenic lead as their end products. In most minerals used for age determination, we find both uranium and thorium and their end products Pb^{206}, Pb^{208}, and Pb^{207}. Thorium minerals free from uranium alone give only one kind of lead—Pb^{208}. A careful analysis of many pure thorium minerals bears out this deduction.

The relationship of these radioactive elements is shown by the three principal radioactive series: the uranium series ending in stable 206 lead, the actinium series ending in 207 lead, and the thorium series of which lead 208 is the end product. All three radioactive series undergo changes that end up in some form of lead. The series starting with uranium 238 disintegrates at such a rate that at the end of 4.5 billion years half of it is transformed and ends up as lead of mass 206. About 1/140 part of ordinary uranium is uranium 235—also called actinouranium—which disintegrates about six times as fast as uranium 238, and it yields lead 207. The series starting at thorium is slower than the rest, its half-life being 14 billion years and coming to an end with lead 208. Natural, or ordinary, lead is a mixture of all three, as well as of a fourth isotope, of mass 204, giving it an average atomic weight of 207.21. Even before careful isotopic determinations were made, Richards and his co-workers had shown a difference in atomic weight between various radioactive leads and ordinary lead.

From these fundamental facts it may be seen that a very careful chemical analysis of specimens picked by a competent geologist leads to age determinations of greater accuracy than ever before attained. Another important consideration is that of the lead isotopes. Formerly the only method available was to estimate the amounts of the different leads from the results of very careful atomic weight determinations.

The work on geologic age determination was correlated through the "Committee on the Measurement of Geologic Time, of the National Research

Council." This committee began its work under the able leadership of Dr. Alfred C. Lane, in the early 1930s, and is continuing under the very efficient direction of Dr. John P. Marble. That so much valuable research has been accomplished, organized, and correlated from several different sciences is largely due to this committee.

As more and more evidence was gathered, the lead method began to carry conviction. There could be little doubt when pure thorium minerals associated in the same rocks with pure uranium minerals gave the same absolute age. The committee, assembling all the data, found that absolute ages fell in their proper relative places in the geologic scale of ages. There were still some ages that did not fall in their proper places, and the method was still not quite perfect; nevertheless, the method and the results obtained from it were accepted by most geologists.

The modern refinements of the lead-uranium method are largely due to the pioneering work of Aston and, later, Alfred Nier in isotopic determinations. Nier made a large number of very careful isotopic determinations of the lead from the same samples that had been analyzed for their lead, uranium, and thorium content to determine a geologic age. Besides these, he made isotopic determinations of ordinary lead from different parts of the world. These careful determinations of the leads greatly increased the accuracy of the methods.

A factor that made for some additional uncertainty was the possibility of lead infiltration into the mineral. Atomic weights did not give an accurate enough figure of the amount of ordinary lead with the radiogenic leads. The results derived from a large number of ordinary lead determinations showed that it always contains a small proportion of isotopic lead of mass 204 and of the stable isotope 206 to the extent of 26 per cent. The isotope 204 is not found in pure radiolead, but occurs in all ordinary lead and seems to have been a part of the chemical elements of the sun when the earth was formed. This gives an accurate, quantitative method of determining whether ordinary lead was deposited in the mineral together with the radiogenic lead.

The early workers in this field were troubled not only by the possibility of infiltration of lead, but also by a loss of uranium together with the lead, owing to the natural weathering processes throughout the ages. Unaltered specimens, usually of a mineral very resistant to these weathering processes, were absolutely necessary so as to be certain that the parent substance and the end products had not been subjected to a change in ratio due to differential leaching. Chemical analysis leads to a high result in the case of oxidized or weathered minerals. As a rule, uranium is more susceptible to leaching than is lead. Early extremely high results can be accounted for by this fact.

Nier's results have shown that we have another indicator of the age of minerals that is not affected by any of the before-mentioned processes; that is, the ratio of the lead isotopes themselves. Isotopes are not altered or their ratio changed by the ordinary chemical processes. Lead of masses 206, 207, and 208 are isotopes with identical chemical properties. Uranium 238 and 235 are isotopes and so are the leads formed from them. There is an indicator ratio of these isotopes that cannot be changed by natural weathering. The ratio, say, in a 400-million-year-old mineral of 206 to 207 lead is 5.5 per cent. This has been carefully checked by Nier. From a Katanga curite, badly oxidized, Nier obtained an age of 610 and 616 million years. The analysis from black unaltered pitchblende gave a result of 617 million years, a remarkable agreement. A chemical age determination, however, on the same oxidized mineral gave a result of 1,000 million years, a value far too high. A careful check value of more of this oxidized material gave a result of 625 million years.

The results of the age determination of a few minerals made by chemical methods, together with the mass spectrographic values, are as follows:

1. Pitchblende from Wood's Mine, Central City, Colorado: 1.063 per cent Pb, 72.28 per cent U, and 0.11 per cent Th. This result gives a lead-uranium ratio of 0.014 and leads to an age of 111 million years. The isotopic abundances of the leads are: 208 is 38.0, 207 is 19.4, 206 is 100, and 204 is 1.01.

2. Pitchblende from Russell Gulch, Gilpin County, Colorado: 38.28 per cent U, 0.053 per cent Th, and 0.643 per cent Pb. The lead-uranium ratio is 0.0167, indicating an age of 127 million years. The isotopic abundances of the leads are: 208 is 44.5, 207 is 22.0, 206 is 100, and 204 is 1.17. The corrected ratio is, then, 0.0146, making it an early Tertiary pitchblende.

3. Monazite from Huron Claim, Manitoba, Canada: 1.524 per cent Pb, 0.28 per cent U, and 15.63 per cent Th. This lead-uranium ratio is 0.257, or, in terms of years, 1,885 million years. The isotopic abundances of the leads are: 208 is 100, 207 is 2.11, 206 is 11.6, and 204 is 0.011. This makes the mineral an early Pre-Cambrian monazite, and one of the oldest minerals.

4. Monazite from near Las Vegas, New Mexico: 0.372 per cent Pb, 9.39 per cent Th, and 0.122 per cent U. The lead-uranium ratio is 0.106, leading to an age of 770 million years. The isotopic abun-

dances of the leads are: 208 is 100, 207 is 1.25, 206 is 10.1, and 204 is 0.028. This monazite was obtained by Mr. Leon Guy, of Las Vegas, New Mexico, from Bull Creek, about fifteen miles from Las Vegas. It is definitely of Pre-Cambrian age, and its age, 770 million years (from the lead-uranium), is close to that found for the Glorieta monazite. The Petaca smarskite of Hess and Wells was also of late Pre-Cambrian.

5. Euxenite from Trout Pass, Colorado: 0.293 per cent Pb, 3.41 per cent U, and 4.25 per cent Th. The isotopic determination of the lead has not been made. From the above figures, leading to a lead-uranium ratio of 0.059, the age is calculated as 400 million years, making it late Pre-Cambrian.

Chemical Analysis for Age Determination

I shall attempt to give only a more or less over-all picture of the details of chemical analysis. The specimen, first of all, must mean something; that is, a mineral whose source is unknown is of little value for an age determination. The specimen should be selected by a competent geologist, so that when its age is determined the formation or stratum in which it occurs is also dated.

After the sample has been obtained, extreme precautions must be taken to avoid contamination, especially by lead. In grinding and sifting the material, no solder or other lead alloy should come in contact with the specimen. All reagents should be tested for their purity, and a known sample of approximately the composition of the mineral should first be analyzed to show the reliability of the method. Recoveries must be made on all filtrates, especially the lead, to avoid loss because of solubility. Tested and refined methods of analysis should be selected. All these and many more precautions must be observed to obtain figures that mean anything, because a slight difference in the percentage of a constituent in some cases may mean millions of years of difference in age.

After the percentages of uranium, lead, and thorium are known, the approximate age can be calculated by the rather simple relationship:

$$\text{Age} = \frac{\text{Pb}}{\text{U} + 0.36\ \text{Th}} \cdot \text{C}.$$

One gram of thorium produces about the same mass of lead as 0.36 grams of uranium, which gives us the thorium factor (the equivalence of the thorium). The value of C most frequently used is 7,600.

This simple formula has often been used and is sufficiently accurate for young minerals, but, contrary to fact, it assumes that constant amounts of uranium and thorium as we now find them were present throughout the entire life of the minerals and in the same relative proportions. For an older mineral it is necessary to use the original contents of uranium and thorium and their diminishing amounts as time passed. This relationship is:

$$t = \frac{1}{\lambda} \cdot \ln \frac{U_o}{U_t} = \frac{\log\ U_o - \log U_t}{0.434\lambda}$$

t is age of mineral (elapsed time)
U_t is present uranium plus the U equivalent of the thorium (U + .36Th)
U_o is original uranium
$U_o = U_t + Pb + He = U_t + 1.155\ Pb$
$\lambda = 1.52 \times 10^{-10}$

$$\therefore t = \frac{\log(U + 0.36Th + 1.155Pb) - \log(U + .36Th)}{6.6 \times 10^{-5}}$$

million years.

A considerable difference in age results from substituting different combinations of constants obtained by various workers in using the analytical data of a certain mineral. This is true, and it is for that reason that the physicist and chemist are concerned about obtaining the analytical data and the constants as accurately as possible. In the case of a Pre-Cambrian mineral, for example, this difference could amount to 100 million years. This fact has led to considerable research in physical and analytical chemistry to develop better and far more accurate methods where these elements are involved.

To the geologist, the absolute age is of some value in determining the age of the earth and the extent of geologic time, but he is more interested in the comparative rather than the absolute age. To him the more exact figures are, geologically speaking, of little importance, and any of the errors mentioned would not bother him too much. He needs for comparison the results referred to a common invariable basis. For this reason, the simple lead ratios alone are all he needs for geological comparison. That is the reason many results are simply reported as "lead-uranium" ratios, and the subsequent calculation of the age in years omitted. To the layman, absolute ages are still of the most interest.

In conclusion, then, it may be said that authentic determinations made by the lead-uranium and isotopic methods show ages in the neighborhood of 2,000 million years. Many of the determinations are from pegmatites and so from a younger geological formation. All the data point to 2,000 million to 3,000 million years as the probable age of the earth's crust.

Duck Botulism

EDITH RUTENIC McLEOD

Edith Rutenic McLeod is an accredited private music teacher in Oregon who spends a great deal of her spare time studying, and promoting local interest in, the natural history of the great Klamath basin. She has written considerably, in a popular vein, on mineralogy, geology, entomology, and allied nature subjects.

WILD ducks and geese are an important harvestable crop in the West, relatively as important as barley, potatoes, and cattle. Considering the fact that "duck sickness," or botulism poisoning, is conceded to be one of the greatest drains upon Western waterfowl resulting from any single natural agency, duck botulism has become a major problem of the wildlife organizations of the West and Midwestern interior.

Four types of botulinum are now recognized in this country, A, B, C, and D. Types A and B are the ones that affect humans and are associated with spoiled canned food such as meats and vegetables. They will also kill animals and birds, as has been frequently demonstrated when spoiled canned food is fed to chickens. Type C, discovered as recently as 1922, is the botulinum that affects ducks and other water and shore birds, especially the ducks that procure their food in shallow water.

Not as much is known about type C as about types A and B. Whereas millions of dollars have been spent by the canning industry in research and equipment to protect human beings from the effects of botulinum A and B, there have never been sufficient funds to undertake the same extensive research into type C.

One of the earliest instances of true duck sickness, so far as can be ascertained with any reasonable assurance, was in the 1890s at Great Salt Lake. Diligent search has been made in the diaries and reports of Western explorers, but no comment on extensive bird mortality earlier than this has been found. These men were all keen observers of wildlife; some were well versed in ornithology. They made frequent mention of the bird life, and there can be no doubt but that they would have noted in their diaries any high mortality of birds in the regions they traversed. It is reasonable to assume,

however, that even in early times duck sickness must have occurred wherever and whenever conditions were suitable. Beginning with the nineties, epidemics of botulism among wild birds have been gradually increasing up to the present time, a situation which coincides with the reclamation of marshlands and lakes, deforestation, and other factors. It is apparent that unless drastic measures are taken waterfowl will continue to decrease until some, perhaps many, of our birds will share the fate of the Labrador duck (wiped out by market hunters in 1878), Ross's goose, the tule goose, the trumpeter swan, and the whooping crane, to mention only a few that are near the point of extinction.

In 1910 botulism among shore birds first received nation-wide attention because of the high mortality among these birds in the extensive marshes of Great Salt Lake. California and other regions also suffered great bird losses at this time. In 1914 the Bureau of Biological Survey inaugurated its first study of the sickness, principally in the environment of Great Salt Lake, and in 1918 summarized its findings: "Duck sickness in Utah is caused by the toxic action of certain soluble salts found in alkali." During the ensuing ten years little research was undertaken, although, as time went on, accumulating evidence threw doubt upon the conclusion that soluble salts found in alkali were the cause of duck sickness. Lead shot, in areas where hunting had been concentrated year after year, and marsh gas were also blamed by sportsmen, but these factors were likewise exonerated.

In 1927 the Biological Survey resumed its work, the first intensive study being made by C. C. Sperry, with headquarters at Klamath Falls, Oregon. E. R. Kalmbach, director of the Fish and Wildlife Laboratory at Denver, Colorado, also made an extensive study of the malady, and in 1930 his work resulted in definitely establishing that duck sickness is a form of botulism caused by a bacterium, *Clostridium botulinum,* type C. This is an anaerobic bacterium that grows in the mud flats as they dry out.

"Duck sickness," "Western duck sickness," "botulism," or "limber-neck," as it is variously called, is a food poisoning, not a disease. The toxin from the bacteria is known to be produced in specific areas under favorable conditions, such as mainly in regions of alkaline soil and water (although alkaline waters are not essential to the development of the bacterium), warm weather, and low water, with fluctuating water flooding mud flats. This coincides with the peak of the dying-off of organic matter in August, September, and October. Decaying animal and vegetable matter serves as an incubating medium for the bacteria, which, though in themselves harmless, produce the toxin that causes botulism poisoning of ducks. Toxin, once produced, does not remain indefinitely in the medium in which it develops but tends to be dissipated or diluted to such an extent as to lose its potency. Ducks do not ordinarily live in areas in which conditions are most suitable for the production of toxin. But when the mud flats have been exposed long enough for bacterial action to take place, a reflooding of these areas forms a shallow "feather edge" that becomes attractive to the birds, and they tend to gather along the newly created shore line.

Botulinum, type C, flourishes in these shallow feather edges of lakes and in the adjoining wet or recently dried mud flats, although it not necessarily restricted to such environments. It is in these shallow waters that the ducks known as dabblers (pintails, mallards, baldpates, shovelers, teal, and gadwalls) take the toxin into their systems as they dip into the shallow water and mud, straining out their food. If reflooding can be prevented by control of water levels, the use of such toxic areas by ducks is negligible. There is little danger of botulism where the water comes up to steep banks, nor is there much danger to diving ducks, coots, geese,

Picking up sick ducks on mud flats of Lower Klamath Lake. It takes a real bird lover to volunteer this service

and other birds that feed in water a foot deep or more, or that browse on dry land.

Endemic botulism is more or less present at all times, breaking out into epidemic proportions when conditions are favorable. Botulinum spores occur frequently in soil samples taken from almost every state in the union, but samples of virgin soil contain the organism more frequently than cultivated soil from the same region. It is therefore evident that these spores are not restricted to filth. Considering the wide distribution and common occurrence of botulinum spores, it is impossible to eliminate them, but it is possible to eliminate, to a great extent, epidemics produced by the toxin among our waterfowl.

Botulism poisoning of ducks is peculiar to the Western states, to certain parts of the Midwest, and to the southern part of the western provinces of Canada. Although botulism is mainly a Western malady, some apprehension has recently been felt in the East because of two small, widely separated outbreaks in Pennsylvania: one in September 1949 in the Tinucum Marsh at Philadelphia, and one in October 1949 on the Susquehanna River below Columbia. There have been other small outbreaks, as in Maine, from time to time.

As was pointed out by the Pennsylvania Game Commission, however, and the U. S. Fish and Wildlife Service, which are studying the situation, there is little danger that the sickness will spread throughout the East, as the conditions under which the bacterium responsible for botulism can thrive are rarely found along the Atlantic Flyway. The acidity that characterizes most waters of the East possibly acts as a deterrent. The natural aeration of the lakes and marshes by springs and streams, and cold weather during the height of migration, are also factors supposedly unfavorable to botulism epidemics.

With a few exceptions botulism seems to affect birds in inverse ratio to their size. Geese are much less apt to be affected than ducks, even though they feed in much the same area. Their browsing and grain-eating habits, in contrast to the puddling habit of ducks, are probably one reason for this.

The toxin affects the nervous system and is characterized by extreme muscular weakness; the birds progressively lose the power to fly, walk, or swim. As the sickness advances the birds lose control of their necks (hence, the term "limber-neck") and sometimes drown if the water is of sufficient depth. Many die of starvation, being unable to forage for food. Because they are unable to hold their heads up out of the mud, leeches enter their nostrils, often suffocating them. The loss of desire for food or water is one of the first symptoms of the malady. The toxin may lead to a slow, gradual poisoning, or the malady may progress rapidly. In the former case the birds seem to sense their illness and instinctively seek the higher shores where, clear of the poison-infected area, some may recover.

Enormous losses of bird life have been sustained in outbreaks such as the one at Malheur Lake, eastern Oregon, in 1925, which claimed a million birds; the one to three million loss at Great Salt Lake in 1929; a quarter of a million birds at Great Salt Lake in 1932; and the 65,000–150,000 loss (as variously estimated by the Fish and Wildlife Service and by California biologists) on Tule and Lower Klamath Lakes, northern California, in 1948. The central valley of California has also suffered heavily. These areas are on the Pacific and Central Flyways, which, therefore, include in the bird loss both migratory and resident birds. Few are the years, if any, when the wild game birds, especially the dabbling duck class, escape epidemics of botulism, although the epidemics vary in intensity from year to year.

The annual waterfowl census taken by the Fish and Wildlife Service shows that the population of these birds has gone down. The millions of ducks and geese that remained in the United States during the generally mild winter of 1949–50 had given the incorrect impression that the waterfowl population had increased. Albert M. Day, director of the U. S. Fish and Wildlife Service, speaking at the 1950 North American Wildlife Conference in San Francisco, said: "It looks now as though instead of gaining ground last year, we lost some of the previous year's gains [in waterfowl population]. We [the sportsmen] not only took the harvestable crop, but also cut into the capital stock of this year's breeders. I fear that the final totals of the recent inventory will show a population decline."

In a few short years the waterfowl have decreased from 250 million to 54 million birds, including a decrease of 26 million in 1946 alone. It is thus to be seen that, although some years show an increase, over an extended period the waterfowl population is steadily declining. Can we afford to lose this many of our game birds through duck sickness and the number shot during the hunting season? Add to this the number of nestlings destroyed by natural agencies, and man-made agencies such as flooding, burning, and drainage of nesting areas, destruction of birds in the fields by haymowers, etc., and the total amounts to a staggering figure that the Fish and Wildlife Service and other conservation groups view with apprehension.

The air-thrust boat, can operate in very shallow water.

Both Lower Klamath and Tule Lakes have long been noted as great waterfowl breeding grounds. Before its drainage in 1909 Lower Klamath Lake covered an area of 75,000 acres and was one of the largest breeding grounds in the West, as well as a concentration center for migratory waterfowl. Tule Lake, before its reclamation, covered an area of 96,000 acres. Only a small portion of it now remains in the southwestern corner. The ill-advised drainage of Lower Klamath Lake forced the birds to seek new habitations, which are becoming pitifully limited in number with the ever-increasing drainage of lakes and marshlands.

A few years ago a tunnel was dug through the hill intervening between the Tule Lake and Lower Klamath Lake basins, and a small portion of Lower Klamath Lake was reflooded with excess water from the Tule Lake sump, restoring a part of the former bird refuge, which, however, is far inferior to the original. Even so, it is figured that 100,000 birds were produced in these two areas in the year 1949, which would be the equivalent of $800,000, if we figure as does the U. S. Fish and Wildlife Service—that each duck and goose has a lumped value of $8.00.

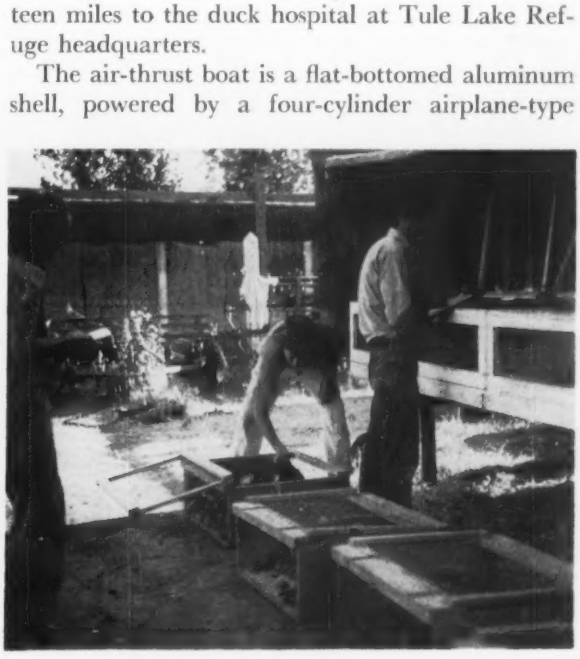
The Lower Klamath Lake Refuge is dependent mainly on excess water pumped from Tule Lake sump. Under ordinary conditions this is sufficient. A number of years ago a series of dikes was built, which divided the district into several pools, with head gates, supply ditches, and canals. This permitted the control of water levels in each pool, allowed the circulation of water through the whole area, or the drainage of each and all pools if necessary. The breaking down of these dikes during World War II, because of lack of funds for maintenance, caused the loss of control of water levels and the serious epidemic of botulism among waterfowl in 1948, and the less serious outbreak of 1949.

As fast as finances will permit, these dikes and canals are being repaired. It is hoped that in the next year or two the project will be completed and that this control program will materially reduce or eliminate epidemics of duck sickness in the Lower Klamath Lake Refuge. Because higher water levels were maintained in Tule Lake in 1949, it did not suffer a repetition of the severe epidemic of 1948.

Although the 1949 epidemic on Lower Klamath Lake did not equal in severity the one of 1948, it was nevertheless of deep interest because of the duck hospital established at the Tule Lake headquarters in August. Howard Sargent, manager of the refuge, carried out a very successful program for combating the sickness. Under his supervision a crew of eleven government men, supplemented by a number of sportsmen, picked up and treated the sick ducks from Lower Klamath Lake, working up to eighteen hours a day if necessary.

An air-thrust boat was used on the lake to pick up the sick birds where the water was too deep and dangerous for wading. Along the shores men sloshed through the stagnant water and mud, often knee-deep, picking up all the sick birds within reach and, where possible, tossing the dead birds up onto the banks so that they would not further contaminate the water. It took the crew two days to cover the twenty-mile shore line of the infected area, which meant an interval of forty-eight hours between pickups on any one spot. The sick birds were put in three or four wire-covered cages on the boat, each cage having a capacity of about twenty birds. When the cages were full, they were landed at any spot along the dike accessible to transportation. Twice a day a truck picked up the cages, five or seven to a trip, and transported them the thirteen miles to the duck hospital at Tule Lake Refuge headquarters.

The air-thrust boat is a flat-bottomed aluminum shell, powered by a four-cylinder airplane-type



Checking in the sick birds.

Injecting antitoxin and giving a duck a drink of water.

motor and propeller mounted in the rear. This is a special type of boat recently designed by the Fish and Wildlife Service for use on the Bear River Refuge in Utah, where conditions are similar to those on Lower Klamath Lake. It can get about in very shallow water and can even travel over mud flats. It is the only one of its type in use on the West Coast, although many of similar design have been used for some time in Florida and other portions of the Southeast, where swampy conditions make them invaluable.

Upon arrival at the hospital the birds were transferred to a double row of raised pens, eight to fifteen birds to a pen. The pens were about three feet square, had wooden sides about a foot high, wire mesh bottoms, and were equipped with covers. The covers were seldom lowered, for the birds were too sick to attempt escape. The hospital is a long, low building, open to the east. The early morning sun reaches the pens, but they are sheltered by the roof from the heat of the day.

Those birds that were too weak to hold up their heads were sorted out, laid on their backs on tables, and given a shot of antitoxin, followed by a second shot if necessary. The antitoxin was injected under the breastbone but not into vital organs. This antitoxin has proved to be remarkably effective, often reviving the sickest birds, and is considered much superior to the former permanganate of potassium treatment. Seventy per cent of the birds so treated recovered. Probably some of the remaining 30 per cent died of starvation, though the sickness may have been checked by the antitoxin. Treatment by antitoxin is expensive. Large ducks were given a 4-ml dose, and smaller ducks a 2-ml dose, at an average cost of eleven cents a duck. Upon arrival each duck was given a drink of water, and twice a day thereafter, until placed in the recovery pen, each received a syringe-full of water. This was done by shoving a rubber tube down the duck's throat and squeezing in the water. Many birds were found with their eyes sealed shut by a mucous secretion characteristic of the sickness. This secretion hardens when the bird is too sick to dip its head in water. The eyes were washed out upon arrival and kept washed out afterward. The last step of the initial treatment was to remove, with forceps, the leeches that clogged the nostrils of most of the ducks, cutting off their air supply. As many as fifteen leeches have been taken from the nostrils of some ducks, though a smaller number was the rule.

Birds that were not too sick recovered in a few days. The sicker ones required a longer period. As soon as they were able to hold up their heads and had regained partial use of their legs, they were placed in the recovery pond. This is a pond of fresh running water, a foot or two in depth, fifty feet long, and with shallow banks. On two sides it is shaded by willows and a canopy, and around it is an eight-foot wire mesh fence. Barley was kept in troughs and scattered in the pond. Most of the birds preferred getting their barley from the water. There was no top to the recovery pond, and when the birds were sufficiently recovered they were free to fly away to the grainfields, or to join the flocks migrating south.

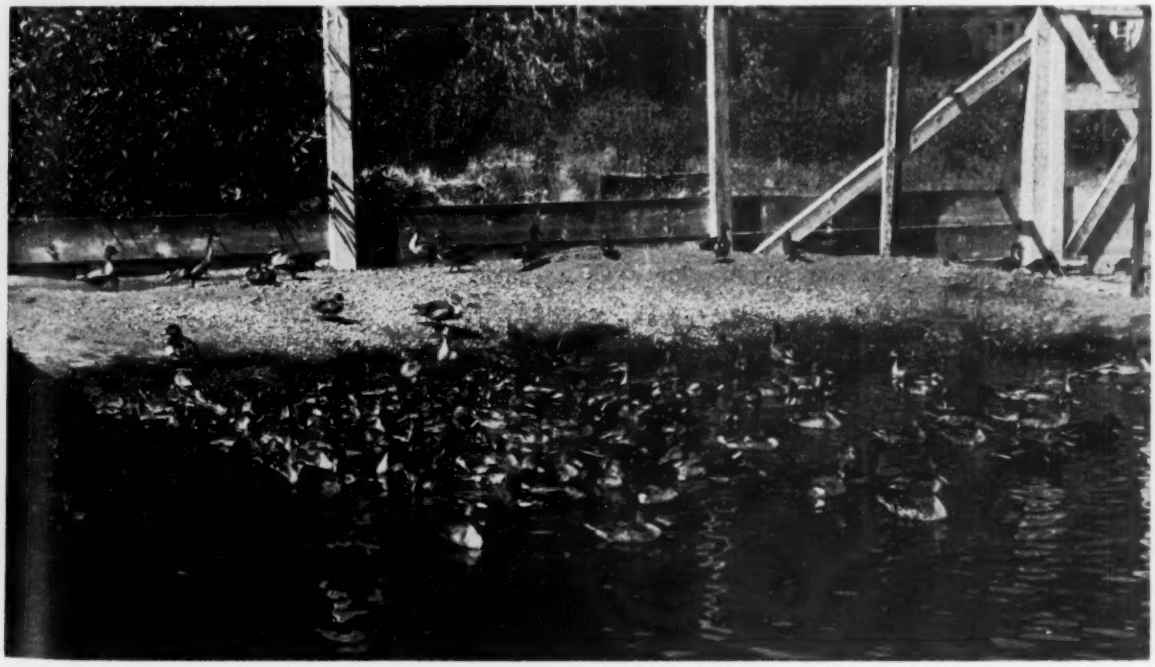
Removing leeches from the nostrils of a duck.

Sick ducks in a pen at the hospital.

While I was at the hospital in September 1949, the truck delivered seven cages containing ninety-nine live birds and about half a dozen that had died en route. There were forty-six pintails, twenty-seven baldpates, sixteen shovelers, four green-winged teal, four cinnamon teal, one ruddy duck, and one gadwall. Usually there are a number of mallards in these pickups, and an occasional redhead and other birds. Botulism also takes toll of shore birds such as sandpipers, avocets, stilts, egrets, yellowlegs, and phalaropes, but few were brought into the hospital, as it was not equipped to care for them. Also, they were more difficult to feed and almost invariably died. Careful records are kept at the Tule Lake Refuge headquarters on all phases of the work, and these records show that of the thousands of birds brought into the hospital 77 per cent recovered. Approximately three hundred a day were brought in.

In addition to the hospital treatment, the picking up of sick birds, and disposal of as many of the dead ones as possible during the pickups, some control measures were taken to keep the wildfowl away from the badly contaminated areas by means of the air-thrust boat, flares fired from rifles, and revolving searchlights. In some cases the shallow water was drained away from ponds, allowing the ground to dry and become unattractive to the birds. After the worst part of the epidemic was over, a crew of men began cleaning out the dead birds from Lower Klamath Lake and disposing of them, as a sanitary measure.

Many people may have wondered about the effect on human beings of eating these botulism-affected ducks. Naturally, no one would eat a duck known to be sick, but there is no way of knowing whether a bird shot during the hunting season may not have recently recovered from botulism poisoning. Only the toxin produced by the activity of the bacteria has a poisonous effect, however, and, in the case of *C. botulinum,* type C, the toxin does not affect man. Thorough cooking destroys it. It is therefore entirely safe to consume ducks that may be suffering from the effects of botulism or that have recently recovered. There can be little doubt but



The recovery pond.

One of several piles of ducks and geese in various stages of decomposition. Gathered from Lower Klamath and Tule Lakes during the 1948 epidemic.

that many ducks suffering from duck sickness have been shot and eaten. It is well known that during the early days of market hunting in California unscrupulous hunters gathered wild birds suffering from, or recently dead of, botulism poisoning, fired lead shot into them, and sold them in the markets.

Fighting duck sickness each year is a strenuous battle; so many birds are lost for the few saved. Obviously the work being done by the Fish and Wildlife Service is a present necessity, but the repetition, year after year, with the expense involved, is in the main almost futile. It is no longer possible to maintain the natural water conditions of the past in the face of ever-increasing demands by agriculture and industry. It follows that in order to control duck sickness the only logical solution to the problem is the appropriation by the government of sufficient funds to allow the necessary construction to control water levels. This requires dikes, drains, and other costly water-control structures. When control of water levels is achieved, it will be possible to alleviate botulism, in some instances, by building water levels high enough so that the mud flats never become exposed and conditions do not become favorable for the production of toxin. These control works can, in other instances, prevent reflooding of toxic areas and the subsequent heavy use by ducks. It is also imperative that we guard against further encroachment by agriculture upon the suitable habitats for wildfowl that we now have; it may be possible, also, to restore some of those that have been destroyed or to improve other suitable habitats.

Gas Laws and Wealth Laws

FRANK W. PRESTON

The author, a native of Leicester, England, has a Ph.D. from London University (1925). He came to the United States in 1921 and has been owner and director of the Preston Laboratories, near Butler, Pennsylvania, since 1928.

EIGHTY years ago, or thereabouts, the law relating to the distribution of velocities among the molecules of a gas was worked out and, from this, the law governing the partition of energy among those molecules. No molecule can have less than zero energy, and very few approach this figure. Many molecules have a modest energy, and a few have very high ones. Since the matter is highly impersonal, no one was greatly offended at this.

The analytical expression is

$$dN = N \frac{2}{\sqrt{\pi} \cdot (kT)} 3/2 \cdot e^{\frac{\epsilon}{kT}} \sqrt{\epsilon} \cdot d\epsilon,$$

where dN is the number of molecules, out of total of N, that have energies between ϵ and $\epsilon + d\epsilon$, when T is absolute temperature, and k is the Boltzmann constant. The graphical expression is given in Figure 1.

The problem was originally one of physics, but essentially the same problem can be posed as a matter of mathematics, with little reference to gas molecules. It is then spoken of as "statistical mechanics," and of course by any other name it would produce the same results. We then have a "canonical ensemble." (R. C. Tolman, *Statistical Mechanics* [ACS Monographs]. New York: Chemical Catalog Co., 1927.)

If it were considered unjust that a few molecules should enjoy exceptional privileges in the form of phenomenal speeds or energies, we might set a Maxwell demon to catch them and take their energies from them. If this energy were redistributed among the less fortunate, slower molecules, it would not stay so redistributed but would promptly revert to the canonical form. This would keep the temperature at its old value. If, on the other hand, the demon confiscated the energy, or a percentage of it, from the faster molecules, or annihilated them, the distribution of the energy among all the other molecules would change. The truncated distribution left after the demon had operated is not canonical and cannot persist. A new distribution, of the canonical form, promptly comes about, but since some energy has been extracted, the temperature falls. The over-all potential of the populations is less than before, and it can do less work, but the distribution of energy among the individuals is as uneven as before.

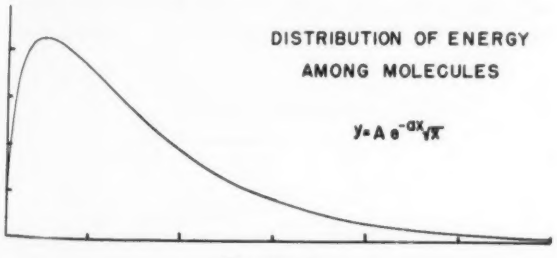


FIGURE 1.

If the demon takes to himself seven others, each worse than himself, and works overtime, singling out the more energetic molecules for the ax, he may ultimately get the energy equally distributed. This will occur when the temperature reaches absolute zero, and no molecule has any energy whatever. It will not occur before.

Around 1895, some time after the Maxwell and Boltzmann laws dealing with gas molecules had been discovered, it occurred to Pareto that wealth tends to distribute itself according to a recognizable pattern. Excluding the very wealthy and the very poor, he found that in all times and all places, the distribution could be very well represented by the equation $N = Ax^{-a}$ where N is the number of people having an income of x units or higher, A is a constant depending on the size of the community (so that N/A is a fraction) and a is an index with a value near 1.5.

In Figure 2 I have taken data not available to Pareto—namely, the incomes of individuals in the United States in the year 1926, as reported in *World Almanac* for 1930. It will be seen that Pareto's law holds very well indeed over a long interval. This law, being much less impersonal

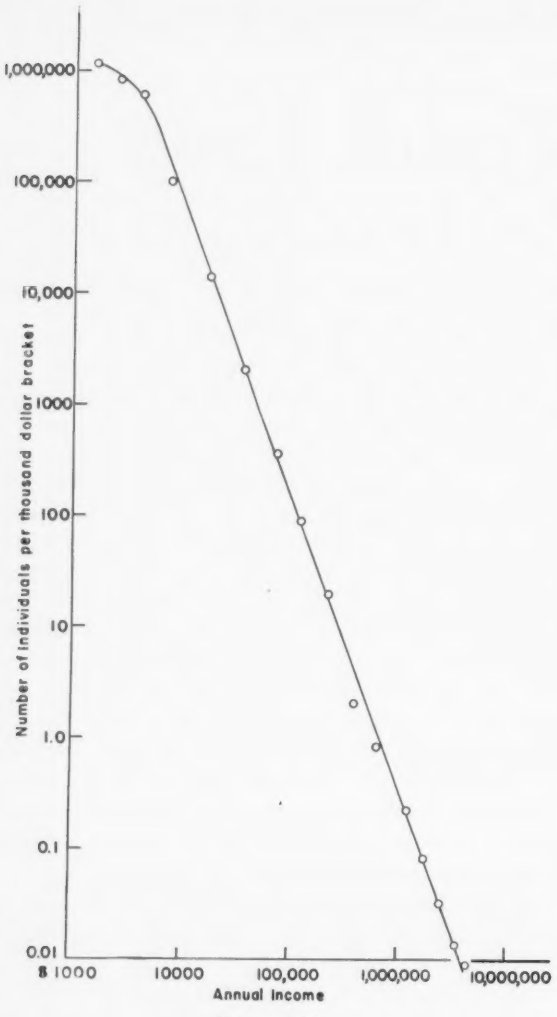


FIGURE 2.*

than one relating to gas molecules, has often been assailed, not on the grounds that it is untrue, but on the grounds that it is unjust. Income, many think, ought to be distributed uniformly, or at any rate *more* uniformly, and many demons have been hired to try to make it so. There is a great difference of opinion as to how much difference of income is tolerable, or perhaps desirable. But it is inherent in the natural distribution that there should be more people with less than the average income than with more. Few people really desire to have less than the average, and so more than half the people can be found in most communities to vote against Pareto's law. One remedy is a graduated income tax, which, pushed to its logical conclusion, will establish equality when everyone has no income at all and the community reaches absolute economic zero.

It has been long known that Pareto's law does not hold accurately for the very rich or very poor (our own curve shows this), and it has been known for some time that when the poor are taken into account the distribution of income can be better represented by a "log-normal" curve (Fig. 3). That is, instead of finding as we go to lower and lower incomes that there are more and more individuals with such incomes, there is a peak in the curve, and then fewer and fewer people have smaller and smaller incomes below this level. This is more or less self-evident, and produces a total national income of approximately the correct amount, whereas Pareto's law, if carried to its logical conclusion, would call for a much larger total income than is the case, by reason of the vast numbers drawing comparatively small incomes.

Pareto's law in its original form does not look much like a law of physics, but a good many things do seem to conform in practice to log-normal distributions. The "normal," or "Gaussian," curve is a very common approximation to the facts in a great deal of experimentation, both in physics and in biology. And where the normal curve fails to fit, there, with a skew distribution, it will often be found that a log-normal curve does fit. The implication is that in trying to fit the normal curve we have chosen the wrong concept to be plotted as abscissa. What ought to be plotted is a ratio, a relation between two things, a geometric series rather than an arithmetical one. This is equivalent to plotting the abscissa on a logarithmic scale; the ordinate is plotted on a "natural," or arithmetic, scale. The curve then loses it skewness and shows up a symmetrical bell-shaped one of familiar appearance.

Attention has been called to this fact by several writers, and in very different fields. I myself have shown (Commonness, and Rarity, of Species. *Ecology,* 1948, **29,** [3]) that in a random collection of insects or birds or other biological material, the

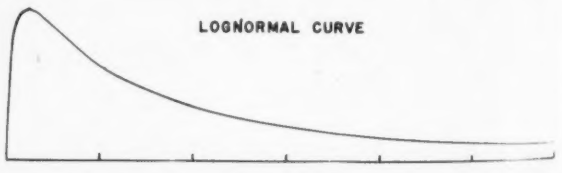


FIGURE 3.

* The method of plotting is slightly different from that implicit in the formula given, since what I have plotted as the ordinate is the number of individuals in each income bracket of $1,000, not the integrated number of individuals having incomes as great as this *or greater.* Therefore, the slope, or exponent, for the straight part, is about −2.5 rather than −1.5. Actually, it is 2.621.

commonness or rarity of the various species tends to plot to a log-normal curve, and a good deal can be explained on this basis.

The richness of a species in individuals bears a certain analogy to the richness of an individual in possessions. Possessions have a way of multiplying just as individuals do. If wealth takes the form of cattle, this is obvious; if it takes the form of invested money, it is scarcely less so. Thus it may not be too farfetched to argue that we might expect wealth, or income, to distribute itself to individuals according to a log-normal curve; and perhaps, if we find that it does, we ought not to be indignant about it.

Perhaps, indeed, we ought not to try to change it. It is not always wise to try to change the laws of nature. Success does not usually attend such attempts, and if success is only to be expected when all life has ceased and nothing moves any longer in the economic system, we should hardly welcome the success. We might do far better to try to understand the law, and the causes that bring it about, than to try to change it out of indignation and without understanding.

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KLZ, Denver	University of Denver (Science Reporter)	3: 15 P.M.
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Seventh Anniversary of Parícutin

FREDERICK H. POUGH*

The author took his Ph.D. in mineralogy at Harvard in 1935, where he was instructor in crystallography in 1934. He has been in the Department of Mineralogy at The American Museum of Natural History since 1935, and curator since 1943. He described "The Fifth Anniversary of Parícutin" in Science (*1948,* **107,** *635*).

ON February 20, 1950, a small group of geologists went to the San Juan observatory to see Parícutin and to mark its seventh anniversary. This gathering was saddened by the death of Mexico's best-known geologist, Don Ezequiel Ordonez; the large group from the Instituto de Geología, which had planned to accompany the party, was delayed by arrangements for a memorial service at the Instituto on the night of the nineteenth. Hence, only Ing. Ricardo Mongés Lopez, director of the Geophysical Institute of Mexico, Carl Fries, Jr., chief of the United States Geological Survey's Mission to Mexico, Frederick H. Pough, curator of physical geology and mineralogy at The American Museum of Natural History in New York, and Mrs. Pough made the trip. Since all the scientists had been there repeatedly, it served as a useful check on the changes slowly taking place, which are particularly impressive when seen only at widely spaced intervals.

Two dominant impressions contrasted with those of two years before, besides the hint of a gradual diminution of activity that will eventually result in the death of the volcano within a reasonable period of time. Most striking, on first sight, is the huge amount of lava that has flooded the valley in which the cone stands. The leaflessness of the surrounding trees and the general desolation now make more impressive the really tremendous lava generation, and permit an instant integration of the entire picture. The view from the Geological Society of America's cabin makes it obvious that the successive flows winding down their separate ways have now filled up the low places so completely that the entire panorama is of a frozen sea of recent lava, lapping the cone that rises in its center. Where there was once an interrupted vista, with tree-covered intermediate ridges rising on either side, there is now an almost unbroken, though irregular, jumble of fresh rock, rising in successive steps to the base of the cone. Lava continues to escape, rising from the same vent—near the old and buried Zapicho—that started two days before the fifth anniversary. It is now a 130-meter mound called Nuevo Juatito, down the northern slope of which the lava cascade is slowly flowing. Infrequent explosive activity and lava breakouts in different directions have gradually built this mound up until it now seems a third as high as the cone itself.

The lava stream that started in the direction of the observatory that the scientists occupied in 1948 continued on that course and piled up ten meters higher than the top of the old crater rim on which the cabin stood. Forming a window in the flow at this point, it spared the now-abandoned cabin from destruction but left it, nonetheless, sunk in a lava pit and inaccessible from any side.

The present flow is proceeding northward to lower ground in the direction of the old San Juan, but its front is progressing extremely slowly, perhaps a foot a day. It has covered again the area where Parícutin lies buried, and now the northern tongue is spreading over fresh ground between the earlier Parícutin flows and the San Juan flow. A western branch is encircling the cone more closely and was still advancing over the jagged tops of earlier flows. Though unapproachable, its rate of advance could be gauged by the clouds of dust that rise as each block tumbles down. It seemed no faster than that of the front that could be visited.

* The writer wishes to express his appreciation to the staff of the Instituto de Geología, whose constant cooperation and friendly assistance have made possible the continuing study of the history of Parícutin, and to Carl Fries, Jr., who made the arrangements for the trip from Mexico City. He also wishes to express his appreciation to Edward Fales, associate editor of *Parade,* whose interest in the developments at Parícutin encouraged the writer to visit it on this occasion.

A trip to the source of the lava, or an ascent of the cone, is now a greater undertaking than it was two years ago. The present approach to the base is from the northwest; but in order to get there, one must first travel about three kilometers in the direction of Los Reyes, to the west of the volcano, rounding the end of the long San Juan flow and then turning back toward the old cone of Conijuato. From this point it is not difficult to climb up along the western edge of the flows and a series of lava terraces to the north side of the base, which lies about three hours' journey from the camp. The route then flanks the volcano with the advancing lava river on the northeast side. Since comparatively few bombs—and those small—come down this side, it is not particularly hazardous. Very much less gas escapes from the flow as it progresses along its bed, so there is none of the difficulty with acrid fumes that has been noted in previous trips up the gully.

The lava cascade shows up brilliantly at night and produces magnificent pictures, but by day it is disappointing. At the time of our visit, on the twentieth, the rate of downward progress was so slow that little surface motion could be detected. Nevertheless, it was obvious from the continuous tinkling and creaking of tumbling and moving blocks that the flow was very much alive. From time to time a fresh red face was exposed in the lava cascade, but usually the daylight view showed nothing but blackness in the "rapids" that glowed so brightly at night.

Gas accumulates in the rising lava column and, on reaching the vent, it escapes spasmodically with a roar and a few ejected lava shreds. The brief outbursts subside quickly without forming lava fountains like those observed on previous trips. The present lava seems much more viscous than before, perhaps because it now contains much less dissolved gas. This is also indicated by the small volume of the fumes escaping from the advancing flow. Only unimportant fumaroles fringe the margins of the later flows. The meanders of the active "channel" in other years were marked by a ribbon of rising bluish fumes (ammonium chloride), and gas vents lined with good crystals of sal ammoniac have been rare since the first year.

The lava progresses in a gradually expanding stream, thirty meters or so wide at the cascade, and spreads out at the base to form the mass of the flow. The surface is extremely rubbly, with blocks of average size over most of the surface. The flow front is about thirty meters in height, sloping back rather steeply. Most of the movement takes place in the upper two thirds of the mass, but some forward pushing along the ground is indicated by a line of small faults paralleling the front approximately six meters ahead of the flow, and by a second higher mound of ash furrowed up immediately in front of the flow. This basal thrusting effect may help explain the failure of the flow to override the 1948 observatory. Resistance from steeply rising ground may well inhibit the free advance of the flow, even though the ground summit is lower than the surface of the fresh lava.

The great lava flow of 1944 that almost covered San Juan appears weakly buttressed by the rapidly crumbling adobe walls. With their reduction, the only permanent monument to the buried town of San Juan will be the façade of the stone church rising above the broken aa surface of the flow.

The huge blocks showing in the lava front are four or five meters in diameter. Their surfaces are dark red, and much of the rubble has a similar hue, presumably from oxidation as the hot surfaces were exposed to the air. Dust rising from the rockslides often exhibits a pink cast. The red shell penetrates several centimeters into the mass, and the rock is rather flaky and friable outside the solid black cores. In megascopic texture and composition, it does not seem to have changed greatly from the lava of earlier visits.

Parenthetically, one might remark here that a series of tongues squeezed out in successive viscous ribbons gives the San Juan flow a surface appearance quite different from that of any other flow. It is characterized by a lava that solidified with large and numerous gas cavities. Even hand speci-

mens of the San Juan lava are distinctive, and not likely to be confused with other lavas. It may have been more fluid and more gas-filled; perhaps that is why it advanced so far, and in so unusual a way.

Explosions from the main vent are much less frequent and less violent at the present time. Most of the bombs ejected are marked on the surface by a glassy froth; their viscous interiors are red-hot and quickly cool to compact, fine-grained rock. The greater number fall back into the crater; those that escape strike somewhere on the sides and progress downward rather slowly. Only a few are large enough to come bounding down with sufficient force to endanger anyone below. Their high viscosity inhibits rounding, either in flight or during their dash down the steep slopes of the cone.

Nevertheless, occasional bombs lying as far away as 1,500 meters from the cone, with typical impact craters, indicate that from time to time the volcano does have rebirths of its early vigor, expelling great bombs to considerable distances. These masses are compact and may be a meter or more across, but, in falling, they shatter and fall to pieces. Seams within them reveal surfaces coated with thin, white gypsum crystals or shiny red opal. Since the bombs presumably were hot and in equilibrium with their environment until they fell, the crystals of gypsum must have grown very quickly, probably between the time they were first ejected and the time that they landed and split open, showing the white, crystal-coated surfaces. Crystallization may also have occurred while many of them were tumbled over and over in the crater before an explosion powerful enough and properly directed expelled them from the crater for the last time.

The seam faces on which the opal was deposited show that they have been attacked by corrosive volatiles, for beneath the opal film the diabasic texture of the basalt is clearly indicated by whitened feldspar laths. The feldspars are uniform in size and arranged in approximately parallel orientation. Former olivine grains exposed on these surfaces are completely corroded and partially filled with opal. Residual fragments of the olivine may be noted only in the largest depressions. Except at the olivine contacts, where a cavity a millimeter or so in depth may have been eaten out, the attack on the feldspar is wholly superficial. The red color in the opal is spotty and due to iron oxide.

The mechanism of the powerful eruptions of the type that eject the far-flung bombs was beautifully demonstrated on the morning of the twenty-first. Around 10 A.M. Parícutin became very quiet, and nothing was seen escaping from the cone. Even the moisture cap, which usually hovers 1,000 meters above the cone at moments when no dust is escaping, had dissipated. This view was so striking that D. Fries took a photograph of the volcano and the

Members of the party, including Mrs. Pough, Carl Fries, Jr., of the U. S. Geological Survey, and Celadino Guttierez, eat lunch on the crest of the levee bordering the lava river that creeps uninterruptedly along the flanks of Parícutin, forming a narrow gully that gives access to the lava *boca*.

Total lapses of eruptive activity, now an increasingly frequent state, are followed by explosive reawakenings that throw an impressive vulcanian plume thousands of feet upward.

clear sky. Suddenly, after about five minutes, the dome of a swelling cauliflowerlike cloud, typical of the explosions of the early days, rushed from the crater, swelling outward and upward in a magnificent plume of boiling gas and dust. Bombs could be seen falling out, stirring up some dust at the base of the cone, and a few seconds later the sudden clap of the vent-clearing explosion reached the ears of the observers. The blast was much louder than was usual for the similarly powerful but uninterrupted explosions of the early days. Following this burst, which apparently resulted from a brief complete closure of the vent, the volcano reverted to the appearance that seems normal for its seventh year of life—a thin rising column of dust, amplified several hundred meters up at the altitude where the moisture condenses, and then, at higher elevations, trailing off with the wind and finally thinning out to nothing. The bluish cast of the condensing vapor, and the brownish filtering of light passing through the gas, indicate that there is still a considerable amount of ammonium chloride and other volatiles mixed with the steam.

The second major impression of the scientific observer returning after an absence of two years is the apparent intensification of the destruction. In the past, the forest had looked to a Northern visitor much like those he had left behind at home, and they were not so obviously forlorn. Now, however, the rotting of the wood has stripped the trunks of bark, dead branches have collapsed under their own weight, and many of the trunks are haphazard piles of shattered wood. Now one senses fully the dismal atmosphere of Parícutin, and the tragedy that this tourist attraction has brought to hundreds of families that formerly lived happily here, making their livelihood from the products of the soil.

Accompanying the collapse of the forest trees, there is an accelerated rate of erosion that also speeds the destruction of the vegetation. Now that the roots of the trees have rotted, there is no longer the anchorage that held back the soil on the slopes, with the result that landslides jumble the tree trunks around, washing branches and whole boles out on the surface of recent flows, and changing drastically the topography of the region. Gullying is very deep and, in places, makes traveling difficult. The slope on which the horses were left on the journey to the base of the cone was cut off by gullies that had grown too deep and steep for the animals to negotiate.

Although erosion is proceeding rapidly, it is, at the same time, having a potentially constructive result. As the jumble of rocks on the surface of the

The shattered trunks of the decaying forest give emphasis to the devastation caused by the volcano, now more apparent than when the trees were still intact and looked like mere dormant giants.

flow becomes veneered with ash and dirt and rotted trees, it forms a basement from which soil can develop much sooner than would have been the case had no such deposition taken place. Since the powdery deposits will tend to remain in depressions and on the flatter areas, it is foreseeable that the earliest restoration will take place on the outer edges of the very flows which, a few years ago, were expected to require thousands of years for a return to forest cover. Aggradation is taking place only near the older, higher slopes, but it will surely be of some importance in the regeneration of Paricutin. The time required to revegetate the landscape will depend in large measure upon the final stages of the volcano's activity. If lava flows continue and further cover the ash deposits of earlier years, while explosive activity diminishes to the point where little ash is expelled from the crater, the final surfaces will have no easily decomposed mantle of soil-forming grit, and the return of life will be slow. Plants will take root only in pockets and crevices where they can find footholds, and barren lava will dominate the scene for many centuries.

Where undisturbed, plant life has made some progress since 1948. The spreading grass, the first plant noted rooting in the fresh ash, has now formed small lawnlike patches in the vicinity of San Juan, wherever walls protect it from the blowing dust. The plant of the tobacco family that grew in the patios in San Juan still survives, as do most of the white flowering fruit trees of the village. The nearer pines and oaks have suffered very badly. At San Juan, however, the woods around the Geological Society cabin are still green, and the ash deposits beneath the trees around the house have a covering of last year's dead pine needles. A few pine seedlings from the last rainy season are growing and feeding in this ash and give every indication of surviving. Out in the open, where seedlings would be exposed to blowing dust and sunlight, nothing can yet live, but in the woods there now seems to be a good chance of survival.

Some rain fell on February 20, and occasional

Accelerated erosion has followed the death of the forests, and old gullies have now been cut far below their prevolcanic depth. Landsliding has developed slopes with crazily tilted trees; subsequent sheet wash has spread trunks and ash over fresh lava, concealing from future observers the true character of the valley floor.

Seventh anniversary cake is cut by Celadonio Guttierez, Observer in Charge, San Juan Observatory, as Ing. Ricardo Mongés Lopez, Pedro Guttierez, F. H. Pough, Lazaro Victoriano, and Carl Fries, Jr., look on.

showers in the dry season may keep the young, protected plants alive. The cabin and the volcano are both situated to windward of a high peak (Cerro de Tancitaro), which appears to be a cloud-former. Hence, although precipitation is undoubtedly greater near Parícutin, it is probably quite unrelated to the volcanic activity.

On dry days the sun still beats down on the bare, ash-covered fields, and stirs up gales of wind and great clouds of obscuring dust. The visitor is likely to find it most unpleasant in such weather. Though it was cloudy and dustless on the nineteenth and twentieth, clear weather came again on the twenty-first, and within a few hours the damp ash had dried and the fine powder was blowing again profusely. The drifting of the ash is an important inhibiting factor to new plant growth in the affected region.

Here and there in the flows, especially near the tourist trails, young plants could be seen springing up, sometimes in the horse dung on the trail, but also in scattered ash-filled pockets in the rocks. They are always in protected spots, where the sun cannot dry them out too fast, and where blowing dust cannot smother or uproot them. Their presence suggests, however, that most of the gases have escaped from the San Juan flow, and natural forces are preparing the way for the return of life.

One other interesting human development should be, literally, recorded. This is the composition, by one of the former residents of San Juan, of several songs relating the history of Parícutin and the destruction of San Juan. Since these are not written down and, without outside intervention, would merely be passed on from one minstrel to another, we have here an interesting example of the contemporary development of native music.

Early Settlement and Land Use in the Present Toccoa Experimental Forest

ERNST V. BRENDER and ELLIOTT MERRICK

Both authors hold positions with the Forest Service, USDA, and are associated with the Southeastern Forest Experiment Station, which carries on research at fourteen centers in Virginia, the Carolinas, Georgia, and Florida. Ernst Brender, branch leader at the 5,000-acre Hitchiti Experimental Forest near Macon, Georgia, spent five years as a forester in the Toccoa area, where he gathered the material described here. Elliott Merrick, station editor, is the author of three novels and a number of articles, travel tales, and stories. The photos are by the U. S. Forest Service.

THIS is a story of the development, peak, decline, and final abandonment of an Appalachian wilderness community in the mountains of north Georgia. The settlement was in the valley of Mulky Creek, which is now within the Toccoa Experimental Forest, administered by the Southeastern Forest Experiment Station of the Forest Service, USDA.

A little more than a hundred years ago white men came to the north Georgia mountain region in search of new land and better hunting. The majority of these people came from Buncombe County, North Carolina; a few came up from Lumpkin County, Georgia. Traders and adventurers had already penetrated the wilderness, but had not established themselves there. The Cherokee Indians still occupied the mountain regions and clung tenaciously to their hunting grounds, although as early as 1832 land grants had been made to white men in the north Georgia mountains, as indicated by the so-called beeswax-and-tallow titles that are still in existence.

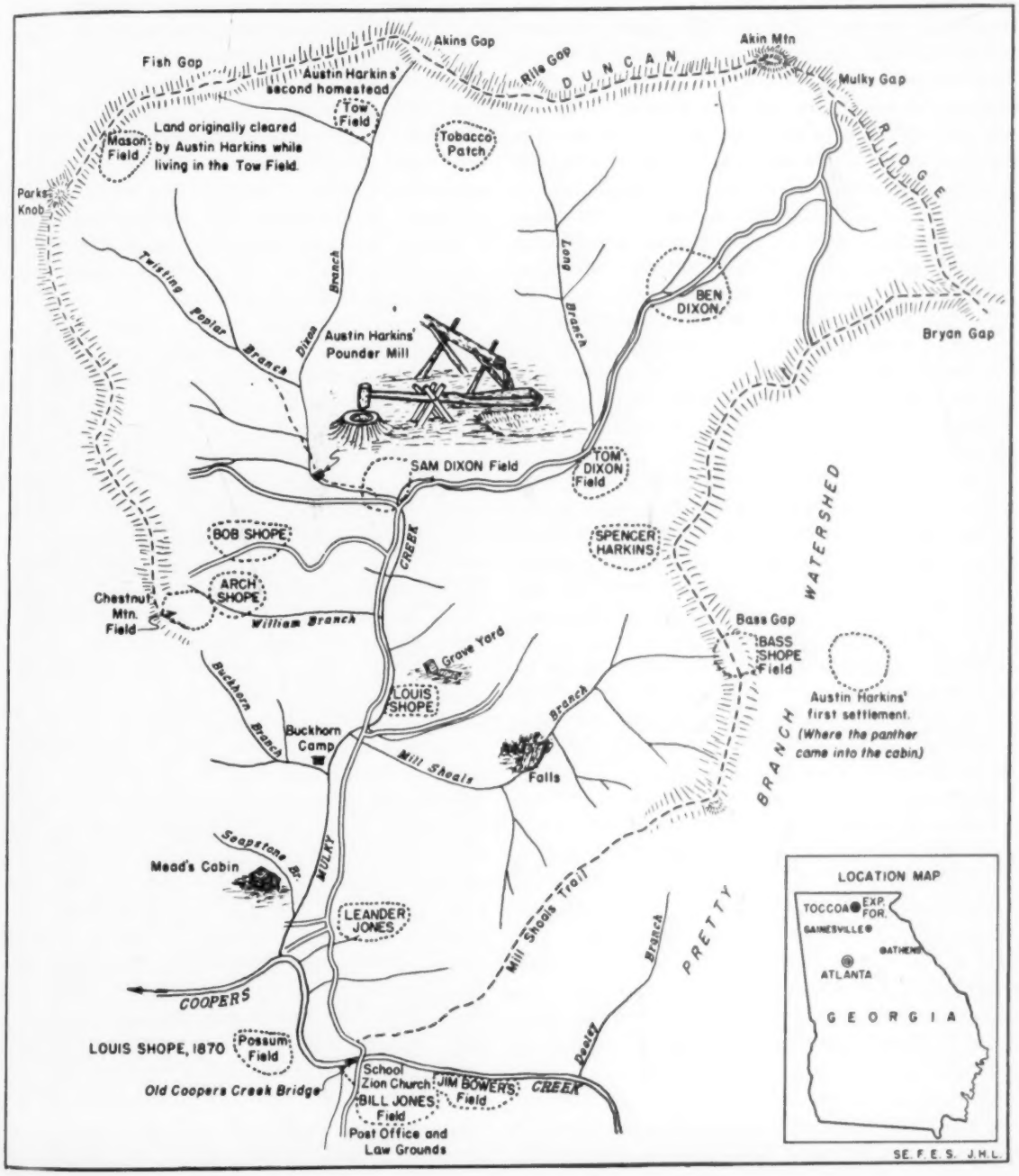
The earliest settlers, arriving while many of the Indians were still in the territory, built their cabins along the main streams, where bottom lands were exceptionally wide. It was not until 1836 that the government transported the last of the Cherokees across the Mississippi into Arkansas. Several of the early settlers participated in this so-called Indian Drive, later to be known as the "Road of Tears," background theme of a drama-pageant given in the mountains of North Carolina in the summer of 1950. The men were hired by the government as teamsters and guards; they later obtained a pension for their services. Two of them, Austin and Berkie Harkins, were the first settlers in the Mulky Creek area. The old fort, one mile below the west boundary of the Toccoa Experimental Forest, where one of the earliest settlers, named Garland, resided, was a gathering ground for Indians in the assembly for the Indian Drive.

With the disappearance of the Indians the region was opened to settlers. The better land along the main watercourses was rapidly taken up by squatters in the years 1840–50, and some of them eventually came into possession of large tracts of land. A certain Charles Davis, who appeared in 1843, owned several thousand acres of wild land along the upper course of Coopers Creek and along the middle course of Suches Creek. This is outside the Mulky Creek watershed proper, but not far away. Davis built a rail fence nine miles long on a side. He owned a large herd of beef cattle, which during that period was an exception rather than the rule. He acquired much of his land by paying back taxes on tax-delinquent land where some squatter had made a few tax payments and then abandoned it. Such land sold as low as two dollars per 160-acre lot. A chestnut bark shanty, a stretch of rail fence, or deadening of a few trees was considered sufficient improvement to give a man squatter rights.

Much of the land in the north Georgia mountains was allocated to Revolutionary soldiers and soldiers' widows by a Land Lottery. They could

draw a lottery number which would represent a land lot number in a certain district, and a beeswax-and-tallow title would accompany the lot number. Much of this land was never claimed and later sold for as little as ten cents per acre. We have been told that one man who came in to inspect his lot was shown the tract by a settler who lived adjacent to it. The old-timer took the man to the most inaccessible places on the land, a group of rock cliffs where a family of chipmunks made its home. The ex-soldier inquired concerning the proprietorship of his land prior to his lucky draw. "Them there chipmunks owned it all," was the reply. The soldier then took his deed and placed it in a crevice of the rocks, redeeding it to the chipmunks. The old settler recovered the deed a few days later and christened the lot his chipmunk lot.

It thus came about that some settlers obtained so much land that they became land-poor, for the income derived from their land would not pay even the taxes. The original settlers valued their holdings and held them for some time, even at the sacrifice



of personal comfort. But eventually they, or in some cases their sons, were compelled to sell the land almost as cheaply as it had been acquired. Charles Davis sold one 160-acre lot for six split-bottom chairs, and another lot he let go for a hog rifle, valued at ten dollars. Ten dollars was, in fact, for some time almost the standard price for a 160-acre land lot.

The Mulky Creek watershed proper was settled during the latter part of the hog-rifle land-exchange period. Berkie and Austin Harkins were great hunters, who loved this wild, undeveloped terrain. Berkie came across Bryan Gap in 1857 with his ox team and all his belongings. He gave a yoke of steers for the Mulky Creek land on which he settled, so it is apparent that someone else had claimed it. Berkie and his boys put up a chestnut bark shanty for temporary quarters while they were building their log cabin. They cut their first winter's supply of hay underneath the high timber along a south slope. Their land was heavily timbered, and before long a logrolling took place. This consisted of rolling or dragging logs from the cleared-off land to a pile for burning, and some of the finest timber in the mountain region was destroyed in this manner. A year before Berkie settled, his brother Austin had squatted near the head of a tributary branch to Mulky Creek. He built, and stayed there until the second year of the Civil War.

The valley was richly stocked with timber, fish, and game in the days when Berkie and Austin settled. There were yellow poplars straight and tall, three and a half feet through at the small end of the first log. Large white pines were found up and down the creeks, and big yellow pines on the upper slopes. White oaks of great size for puncheon boards were to be found everywhere. The woods were clean then and there was little undergrowth. Broomsedge on the south slopes grew shoulder-high, pea vine grew waist-high everywhere, and so did red root. Beggar lice were likewise abundant.

Game was plentiful, and deer could be seen everywhere. Prior to Berkie's settlement at the mouth of Millshoal Branch, a chestnut bark camp was located at the Buckhorn Branch, which enters Mulky Creek opposite Millshoal Branch. The camp was known as the Buckhorn Camp because antlers were piled waist-high at the campsite where hunters had skinned and dressed their deer.

Turkeys and grouse were very numerous; eagles could be seen quite often, and wood pigeons came through the gaps into the valley and settled upon trees in such great numbers that the limbs broke. Ravens and blackbirds added their share of noise. Wolves could be heard howling at night, and an occasional panther cry quivered through the darkness.

On one of their earlier hunting trips into the Mulky Creek area, Austin and Berkie came upon a litter of wolves in the rock cliffs to the east of Mulky Gap. Because it was getting dusk, the men were afraid to enter the lair, so they built a large fire outside the den and spent the night there while the she-wolf prowled around clicking her teeth till day broke and the men killed her and her litter.

A panther one evening entered Austin Harkins' cabin, which was then located on the Pretty Branch side of Bass Gap. The door to the cabin stood open, and Austin's wife sat spinning by the light of a grease lantern when the panther entered. The animal would not approach the light but squatted in a corner of the cabin flaying the puncheon-board floor with its tail "till it heard the menfolks approaching," when it dashed back into the woods. No doubt Mrs. Harkins then burned feathers from a feather bed in her chimney place, which was the approved procedure for keeping panthers and wolves away from a homestead.

Upon occasion a bear would plunder through the valley, and stories of these raids are still remembered. 'Coons and opossums, squirrels and rabbits, too, were plentiful in those days. Ground squirrels, in particular, were very numerous, as the chestnuts provided more food than they could eat. The streams were stocked with speckled trout, and the land was a paradise for hunters. In the fall of the year the settlers would go on a big hunt to shoot deer and turkey to salt away for winter use. Tate Smith, who resided at the old Coopers Creek bridge from 1885 till 1896, made it his special duty to shoot a deer each Sunday. The excellent hunting on the Mulky Creek drainage was without doubt the main reason for its settlement.

Cultivation by means of a single foot plow followed the logrolling. The plow was a homemade affair, manufactured entirely of wood. One had to go over the ground several times before the land was ready for sowing. The depth of cultivation was from two to three inches. The first crop of corn averaged about ten bushels to the acre, the second year as high as twenty and twenty-five bushels would be harvested, but soon the native fertility of the land grew less, so that on the average seven to ten bushels of corn would be made to the acre. A large proportion of small grain, such as wheat and rye, was raised by the early settlers. "To make good whiskey always put a little rye with your corn," they used to say. There was good profit in making whiskey, since a bushel of corn, which sold for fifty cents, brought $1.50 when converted to whiskey.

With the land under cultivation, a pounder mill to grind the corn became imperative. So Berkie erected one below the shoals (falls) on Millshoal Branch. It was an ingenious affair, with a long balance bar made from a log that was scooped out at one end to hold water from the small overhead jet that powered the mill. As water filled the scoop, its weight made that end sink, raising the hammer at the other end of the pivot. With its sinking, the scoop emptied itself, allowing the hammer to fall. Then the scoop filled again.

Following the settlement of the Harkins brothers, other people rapidly took up the remainder of the wild lands. One of them, Leander Jones, came in 1863 during the Civil War and was the first settler upon the present Toccoa administrative site. This first homestead was erected in the lower end of what we call the White Pine pruning lot. When his family expanded, he built a new house near the spring at the present Toccoa headquarters. Leander was an ardent target shooter, and he participated in every shooting match held within walking distance. A shot at a turkey at sixty yards from a prone position, or at thirty yards standing, was to his liking, or a shot at a bull's-eye for beef cattle was equally welcome. On one occasion he put up his homestead in a shooting match and lost his home, land and all, to a better shot. In later years his wife testified to this during a revival meeting.

Second-growth white pine about fifty years old, near Mulky Creek.

Settlement was only slightly interrupted during the Civil War. The two Harkins brothers were Yankee sympathizers and did not fare well in the valley. Their stock were killed and driven off and even their lives were in danger. Some time in the second year of the war, in the dead of night, they put their belongings into wagons and, dodging the home guards, struck out for the North. In Tennessee Austin stayed behind, sending his children northward with his brother. Austin's wife had died before they had left the Mulky Creek valley. On his journey through Tennessee he embarked upon another courtship and lost track of the whereabouts of his family for six months. When finally he located them in Indiana after the surrender, they returned to the north Georgia mountains. Berkie went back to his former homestead, and Austin settled upon the flat area now known as the Tow Field. Austin cleared some land on the lower north slope of Parks Knob, and put up a pounder mill below his field on Dixon Branch. Austin's settling at the above place marks the end of the first stage of the Mulky Creek settlement. Another generation now became active in the valley.

The sons and daughters of the original settlers had become grown men and women. They married and spread out upon homesteads of their own. More logrollings took place, more timber was cut to build cabins, the poleax and broadax doing all the cutting, as there were then no crosscut saws in the country. The first saw was introduced in this valley in 1888, when another man and Payton Harkins, son to Austin, bought one between them. They prized their saw highly and would not lend it to anyone. With the introduction of the first saw, others soon followed, so that by 1890 everyone was in possession of a saw, and timber fell faster and more easily than ever.

One after another, the grown sons established homes of their own, some of which are still marked by the remnants of a chimney. Most of them had sheep, cattle, and hogs pastured at random in the unfenced hills. All cultivated land was then kept under rail fence, evidences of which remain today. About six acres of land were tended per grown man in a family; thus a man and his grown son often kept about twelve acres under cultivation.

The Shope family made its appearance in the

This curly yellow poplar (tulip tree) in the Toccoa region contained 10,900 board feet and sold in 1931 for $317 on the stump.

valley in 1867. They were acquaintances of the Harkins, and were likewise staunch Republicans. They, too, had gone to Indiana during the war, where they had met Berkie Harkins and his family. They and their descendants eventually fanned out into eight or nine different homesteads. The most capable was Louis Shope, who married Leander Jones' girl, and bought out Berkie Harkins in 1870. He eventually acquired some five hundred acres of land within this valley. Louis was a hustler. He tended all the land that Berkie Harkins had cleared and broke new ground two or three miles from his homestead, such as the Chestnut Mountain field situated above his brother Arch's place, and the Possum field south of the big bend on Coopers Creek. And still the Shope family had not reached its full representation in the valley; Bass Shope, a sixth brother, moved to the Ben Dixon field in about 1880, but he was not the original settler in that place. As new ground was broken here and there, bottom land became scarcer, and people started scratching into the subsoil.

The saturation point of the land had not yet been reached, but it was close. A few settlers still drifted into the valley, but in general they took over established farms. To supplement the reduced fertility of old ground, they would break additional new ground, mostly on land with a northerly exposure. Steepness of slope was not considered much of a detriment. A number of steep north slopes were cleared off and annually produced about eighty bushels of corn per five-acre patch for five or six years, till the topsoil washed off.

It was during this period that the woods were heavily grazed by livestock, in particular sheep and hogs. The sheep furnished wool for homespun clothing and mutton for the table. It is said that some of the old-timers had a steady mutton scent about them. Hog meat and venison, plus cornbread, helped to keep body and soul together. Every family had from ten to twenty sheep, and an equal number of hogs. Louis Shope owned a flock of one hundred sheep and just as many hogs. Spencer Harkins had seventy-five sheep and hogs. Tom Dixon owned sixty sheep and perhaps two hundred hogs. Cattle, too, were becoming more numerous. To provide tender young grass and herbs, the woods were burned annually, and the combination of burning and grazing took a heavy toll of the land.

Some of the new residents were rather shiftless, having neither land nor ambitions of their own. It is told of a certain Jim Bowers, who resided here and there in the present Experimental Forest, that upon approach of a wagon his rooster would lie on his back and stick his feet in the air for tying, in the expectation of a move to a new home.

The Mulky Creek settlement was a thriving community during that time. In the year 1876 a schoolhouse and church were erected at the old Coopers Creek bridge. The school had an enrollment of seventy-five pupils from Mulky Creek and adjacent small watersheds, such as the Pretty Branch and Davis Creek. There were also Law Grounds at the old bridge, as well as the Polk Post Office. The Law Grounds were for trying petty law cases, collecting taxes and votes, and conducting other public business.

There was a quick turnover in land ownership during this period. Some people preferred to rent land, and others were just not of landowner caliber. Payton Harkins used to get drunk and climb a high poplar tree to its very top, then sway and rock the tree as hard as he could, in spite of his family's entreaties to come down. He would threaten suicide up there, and stay with his jug for hours at a time.

The topsoil of the land was washing away insidiously and rapidly. The old-timers had used cover crops during the winter months, but the later settlers

took less interest in the land. The clearing of new ground was easier, now that they had crosscut saws.

The double foot plow cut a deeper furrow into the soil and exposed more of it to the agencies of erosion. The method of hoeing corn also contributed its share to the loss of topsoil. The settlers would start hoeing corn from the top row of a field, rake the weeds down into the second row and on down till the bottom row was reached; the field then looked as clean as a cabbage-plant bed. The average life of a cleared slope under this method of cultivation was five to eight years, depending on the steepness of the slope. There is little gully erosion on most of the land in the north Georgia mountains, it being mostly a matter of sheet erosion, of a subtle type, which is not recognized till the low productivity of the soil makes it evident, or till the rocky B horizon is reached.

As has happened in many parts of the United States, land values rose in spite of the fact that fertility was declining. Sam Dixon in 1896 paid three hundred dollars for two lots of land, one a so-called mountain lot, the other a bottom-land lot. In 1903 he sold the bottom tract to the Pfister and Vogel Land and Leather Company, of Milwaukee, for two hundred fifty dollars, and the mountain lot sold for one dollar per acre. Louis Shope sold out to the same company at two dollars per acre for both bottom and mountain land. The Forest Service eventually paid three to five dollars per acre.

The timber on the land began to assume some value. Possibly this accelerated the exodus of people that was in full swing by the 1890s. A little ready cash could be obtained by selling out to the lumbermen. In 1905 a logger by the name of Mead came into the valley from New York and settled briefly. Mead erected a cabin on top of the ridge to the west of Soapstone Branch. He bought the finest yellow-poplar timber at the price of one dollar per tree. He also cut the best of the pines, both white and shortleaf. The logs were floated down Coopers Creek to the bridge at the present Toccoa River highway, where a boom assembled them. Mead stayed only a few years, till the best timber along Coopers Creek was logged.

Another inducement for abandonment of the land was cotton, the cash crop of the South. The Piedmont cotton section is now full of farmers who came from the mountains some forty-odd years ago. Some of the stamina that brought the Harkins and the Shopes into the valley carried them on into the cotton country in search of a better means of livelihood.

Still another factor exerted a drain upon the settlement of the smaller valleys. The communities along the main waterways, where the land is less steep and the bottom lands are wider, soon enjoyed better roads and better stores, better schools and better churches. In all, the people drifted away from the Mulky Creek area because the land was depleted; the Pfister and Vogel Company recognized the value of timber and gave ready cash for the land, and this money facilitated family removals to the beckoning land of cotton; and the more tenacious finally moved out because community life was too remote. Place after place was sold or abandoned. The school at the bridge had been given up as early as 1885, and the post office in 1896. The Pfister and Vogel Company extended its holdings and bought out most of the remaining settlers between the years 1903 and 1909. The last man in the valley sold out in 1914.

The Pfister and Vogel people kept a man at the Louis Shope place to protect the woods against fire and trespass; he was allowed a small patch of land for cultivation, but no new ground was to be broken by him. Thus the land that was first broken and put into cultivation by Berkie Harkins in 1857 was last abandoned from cultivation in 1919.

Volunteer yellow poplar on an old oil field in the Southern Appalachians.

Virgin hardwoods in a Southern Appalachian cove.

It is interesting to speculate on the effect that clearing, domestic use of timber, and woods burning and grazing have had on the forest.* We estimate, for instance, that the direct demands on the forest for timber products by settlers during their sixty years of activity amounted to about half the growth. It must be remembered, however, that high-quality trees were as a rule the ones used. Even for firewood the settler had every incentive to cut the straight-grained, knot-free trees that would split quickly and easily. Thus, in this area, recognized as one of comparatively good old-growth timber, a light culling of the highest-grade trees took place over a period of sixty years.

As to fire, it may be conservatively estimated that, for a period approximating two generations, the forest was influenced by the slow attrition of occasional burning. Exact statements as to the results of such burning cannot be made, but scars now present at the bases of many trees indicate some loss of volume in butt sections. Of probable greater importance was the killing of at least a part of the reproduction, so that certain size classes are almost entirely lacking in present-day stands.

Grazing effects add up to somewhat the same "indirect" result. During the three decades of highest population, an average of thirteen homesteads was occupied, each running from ten to twenty sheep and an equal number of hogs in the woods. Some families pastured more heavily than this and had a few work animals. At a conservative estimate, some five hundred head of stock grazed the uncultivated 3,330 acres of the area for a generation. That would be an average of one head to about seven acres of woodland range. Both before and after the one generation of peak development in the valley, grazing took place on a reduced scale. This continued to as late as 1936. The influence of such a grazing history upon the forest cannot be determined now with any accuracy. Much is known, however, of the general effects of continued, intensive grazing upon hardwood forests. The result is gradual deterioration of the stand caused by the natural death of the overmature trees for which replacements are not available, they having long since been browsed out. The final result is an open stand of large, overmature, and rapidly dying trees, with a forest floor clear of reproduction. Such extreme cases are rare in the Southern Appalachian region, but the grazing history of the Toccoa Experimental Forest indicates that for the period 1866–95 the development of reproduction was severely hampered by the browsing of sheep and the consumption of mast by hogs. For the past forty years grazing has probably not been a significant influence in forest development.

As the picture can be reconstructed now, the "indirect" forest influences—grazing and fire—tended to retard development of new growth that would otherwise have been available to replace trees cut for domestic use or those dropping out of the stand from natural causes.

In the removal of 25–30 per cent of the standing timber volume by the chestnut blight, and in the influences of settlement, both direct and indirect, lie the answers to many questions that enter the observer's mind when he notes the low volumes, high percentages of defect, and general low stocking of "uncut" stands on the Toccoa Experimental Forest. General observation of other areas still untouched by commercial logging operations leads to the conclusion that such a history has been fairly common in the Appalachian hardwood types.

Since the settlers left, the area has made a considerable comeback. Natural seedlings filled the openings, and erosion has practically stopped. Many

* Acknowledgment is made to Leonard I. Barrett, of the Forest Service, for valuable help in interpreting the influence of past land use on present forest condition.

of the old fields seeded in naturally to fine stands of pure white pine and yellow poplar.

There are those who think that the Toccoa history represents a picture of a flowering, prolific, abundant, but delicately balanced land that was so quickly despoiled by man's needs and carelessness that it was also quickly abandoned. This is not entirely according to fact. It is true that the valleys considered here were rather rapidly transformed from a wilderness rich in game, fish, timber, and soil to a collection of "skinned" mountain farms. The soil, vegetation, and natural adjustments are delicately balanced in this land of high rainfall, "gully washers," hot summer sun, open winters, and erodible subsoil. But the people did not leave primarily because the land had been extensively depleted in less than a generation. They left primarily because there were greener pastures, they thought, in the Piedmont, because there were high wages elsewhere, because there were no industries to turn to for part-time wages, and because principal transportation routes had by-passed the tributary valleys to the Toccoa River region. Even ten years ago the dirt roads were so bad that the twenty-five-mile log haul from the Toccoa Forest to the nearest mill was prohibitively expensive. Many of these factors are so interdependent that it is impossible to say which is a cause and which an effect.

As with much of history, it is futile to try to fit Mulky Creek to a neat pattern. The people came, stayed awhile, and for a variety of reasons went away again. The trees remain, and that is good, for the soil recovers its fertility under trees in this natural forest land.

STUFFED STORM PETREL

Mounted mothbait, dust-gathering
In an Academy adorning
The T-joint of the prairie sewers,
This desiccated air was never yours.

You breathed and beat the buffeting
Element that tossed you crumpled at the foot
Of a tower that held its spinning vanes
With anchored root.

Did the same breath whose blast put
You astray on the windy lanes
Blow in your burning lungs their courage?

The power that cut your pilgrimage
So short darkly impels us all.

When strange wings cleave a thinner air,
Perhaps the puzzle of our fall
Will stimulate
An alien stare
As curiously compassionate.

WILLIAM NEWBERRY

The Early History of Rocket Research

JOSEPH W. SIRY

The author is acting head of the Theoretical Analysis Section of the Naval Research Laboratory's Rocket-Sonde Research Branch, and has been associated with it since it began to utilize large rockets for upper-atmosphere research in 1946. For our Mid-Century Issue (December), Mr. Siry has written the history of rocket research in the first half of the twentieth century.

ROCKET research in the United States today is a large-scale attack upon a problem of considerable importance to the nation. In a manner typical of the modern approach, millions of dollars are spent each year to support the work of thousands of scientists and technicians engaged in advancing the rocket art. The research entails studies in many fields, including mathematics, physics, chemistry, metallurgy, aerodynamics, and even cybernetics, one of the newest of the disciplines.

Although sponsored almost entirely by the three arms of the Department of Defense, much of the research is actually carried out by private agencies. A number of universities have been attracted to the field. Important work also is done by industrial establishments. Giant corporations such as General Electric, whose names are household words in other contexts, are active in many aspects of the program. As might be expected, aircraft manufacturers such as Douglas and Martin play prominent roles, particularly in those phases that have to do with aerodynamics and structures. The rocket propulsion units are often developed by newcomers to the corporate ranks. For example, the Aerojet Engineering Corporation and Reaction Motors, Incorporated, which were founded within the past decade, have contributed much to the American research and development program, yet their names are hardly known outside rocket circles.

Rockets and rocket research equipment are studied and developed in the vicinity of the world capital in New York, in the environs of the national capital at Washington, and in a number of other cities in different parts of America. The work goes on not only in our greatest metropolitan centers but also in some of the loneliest and most remote corners of the globe. In many ways, the White Sands Proving Ground is the capital of rocketry. It is here that the final assembly, preflight preparation, and launching of the largest rockets take place. This firing range (Fig. 1) is operated by the Army Ordnance Department. It is more than one hundred miles long and includes over four thousand square miles of the New Mexican desert, but need for much larger testing areas is at hand. Only the oceans and desert stretches of continental proportions, such as are found in Australia and Africa, remain as practicable sites, and even these taken singly will eventually be inadequate. Large rockets are already being fired into clear areas at sea, both from naval vessels and from coastal bases. Plans were laid as early as 1946 for a three-thousand-mile range to be located in Australia and in the Indian Ocean. The continental portion alone will be two hundred miles wide and more than a thousand miles long.

The support that rocket research currently enjoys was not always so generously given, however. It is scarcely a dozen years since the field was almost exclusively the province of a few zealots working with relatively meager resources. The scale of rocket research operations was correspondingly smaller. In the early thirties the thrust of experimental motors was measured in pounds rather than in tons. In those days not only the construction but even the flight-testing of rockets often took place in large cities. The *Verein für Raumschiffahrt* ("Society for Space Travel") launched experimental missiles from its *Raketenflugplatz* ("rocket airdrome"), located in Berlin. As recently as sixteen years ago a rocket traveling at almost the speed of sound was actually being flight-tested in New York City by the American Rocket Society! This rocket is shown in its launching rack in Figure 2.

The rapid growth of rocketry in recent years is due in no small measure to the contributions of such men as Goddard (*cf.* Fig. 3) and the small bands of

enthusiasts who formed the nuclei of the rocket societies in the twenties and thirties. They not only worked out a considerable portion of the basic theory, they also solved many of the vexing practical problems that invariably arise in this type of pioneering development. What legacy of knowledge and experience did these men inherit when they took up their work? The answer is to be found in the early history of rocket research. Before tracing this history, however, it is important to have a clear notion of what the rocket is, and what it is not.

Two essential concepts distinguish the rocket from other vehicles. One is the jet reaction principle, which has been known experimentally for more than two thousand years, but which was not given formal explanation until Newton enunciated his third law of motion in 1687. The other is the fact that useful exothermic processes can proceed without the aid of atmospheric oxygen. Knowledge of primitive chemical reactions other than elementary oxidation also antedates the Christian era, but more than two thousand years were to pass before such phenomena began to be clearly understood.

Propulsive devices that exploit the consequences of Newton's third law of motion by employing a jet are often called reaction engines. Those reaction engines that rely upon the atmosphere for their supply of oxygen are usually known as jet engines. One of the most spectacular of these was the pulse jet that drove the German V-1. Other important types are the turbojet, which is widely used in aircraft applications, and the ramjet, or athodyd, which is coming into use in the guided missile field.

Rocket engines, on the other hand, are jet reaction devices designed for vehicles that carry all the fuels needed to support the propulsive process. These fuels are usually either liquids or solids. In general, a given rocket will employ either solids or liquids, although at least one rocket has been designed to use both types of fuel simultaneously. The

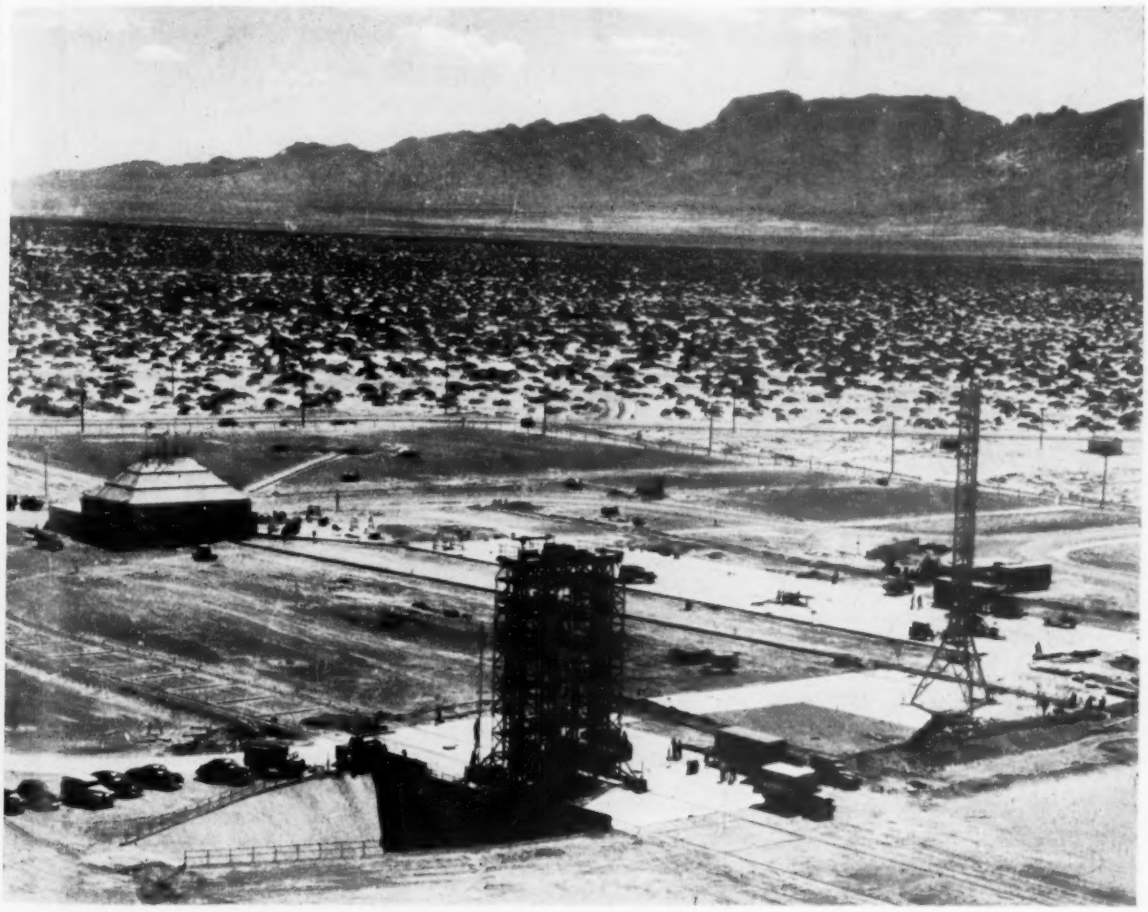


FIG. 1. The first Viking standing at firing pit, White Sands Proving Ground. The gantry is in position over the rocket, where it facilitates the installation and checking of upper-atmosphere research equipment. The blockhouse is at left, and the Wac Corporal launching tower is at right. (Official Department of Defense Photograph.)

distinction between solid- and liquid-fuel rockets is not a trivial one. Their characteristics differ in a number of important ways. This is partly the effect, and partly the cause, of the dissimilarity between the objectives that stimulated their development. The powder rocket, which preceded the liquid-propellant rocket by almost seven centuries, was usually envisioned as an alternative to the arrow, and later to the gun. A great many of the forward steps in powder rocket development were taken in response to military urgencies. Often a particular application in view, or the result of a specific engagement, has stimulated a new spurt of effort.

The development of liquid-fuel rockets was begun for quite different reasons, however. The knowledge of the lower atmosphere acquired during the nineteenth century brought with it an interest in the higher strata. Balloon flights for scientific purposes were begun toward the end of that century. A search for means to carry instruments to even higher altitudes ultimately led Goddard to the liquid-fuel rocket.

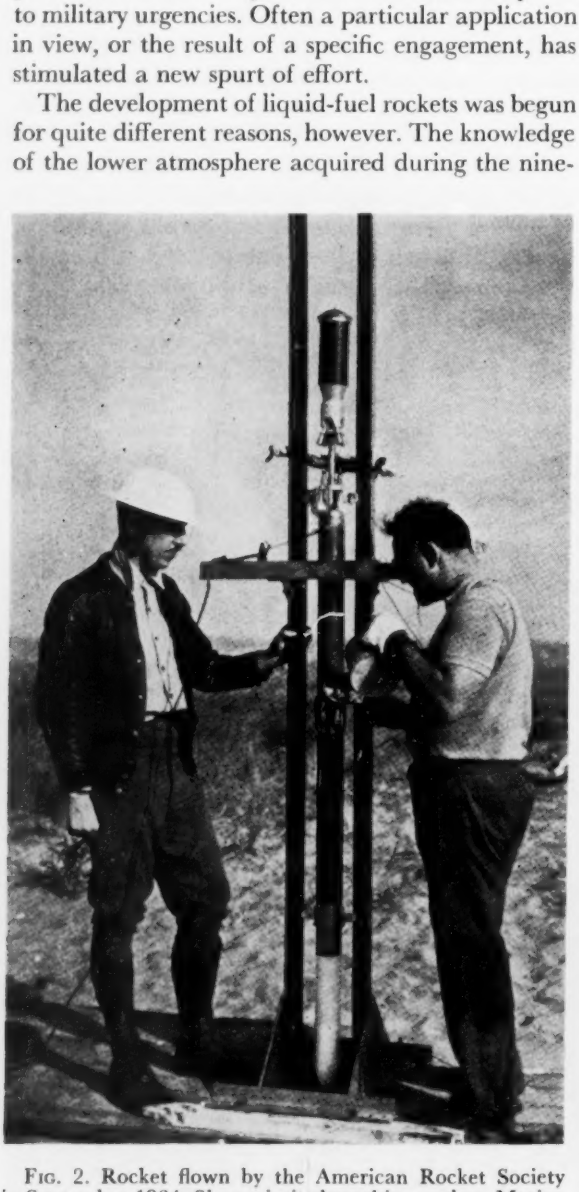


FIG. 2. Rocket flown by the American Rocket Society in September 1934. Shown in its launching rack at Marine Park, New York City. John Shesta, the designer, is at the left, and G. Edward Pendray, at right, is pouring fuel into one of the tanks. This rocket reached a speed of more than 1,000 feet per second in the flight test. (Reproduced by permission from Pendray's *The Coming Age of Rocket Power*. Harper, New York, 1945.)

Curiosity about the upper air was not the only spur to the development of the liquid-fuel rocket. The idea of travel through interplanetary space also was given serious thought in several countries at about the same time. The experiments of the American Interplanetary Society and the *Verein für Raumschiffahrt*, as the names suggest, were carried out with this long-range goal in mind.

The distinction between powder and liquid-fuel rockets is not only a technical one but to a certain extent it also reflects both a difference in function and a chronological separation. Accordingly, the history of rocket research begins with a consideration of the factors involved in the development of powder rockets.[1-7]

The Beginnings of Rocket Research

The discovery of the pyrotechnic composition was probably the first addition to human knowledge that may properly be considered as forming part of the history of rocket research. It is now generally agreed that Greek fire was such a mixture. It was composed of sulfur, pitch, resin, and crude saltpeter. The armies of Alexander the Great were among the first to feel the effects of this strange new substance. The first reference to it is probably one that appears in a chronicle of his Indian campaign. It was related that, in the year 326 B.C., the defenders of a town in the Punjab on the River Beas used "lightning and thunder, which darted upon their besiegers."[2]

The second milestone was the invention of the aeolipile, which took place about two hundred years later. The first aeolipile, built by Hero of Alexandria during the second century B.C., was a jet reaction device operating somewhat like the familiar lawn sprinkler. Its central feature was a spherical or cylindrical vessel that was free to rotate about the horizontal axis defined by a pair of trunnions. One of the trunnions was hollow, to permit the entry of steam from a stationary boiler. The vessel was made to rotate by two jets of steam issuing tangentially from a pair of oppositely mounted, L-shaped nozzles lying in its equatorial plane. It was in east-central China more than a thousand years later that these two basic notions were combined in the form of the first actual rocket. Speculation as to how it was created has given rise to the following general picture of what probably happened.

The first rocket evolved from the fire arrow, a standard item in the arsenals of the early Christian era. This weapon often failed of its incendiary purpose because the flame was extinguished in flight by the rapid motion of air past the flying arrow. In casting about for a better fuel, it was recalled that the addition of salt to burning materials gave a brighter flame. If the flame was brighter, it should also be more persistent, according to the logic of those times. In inland regions such as central China, saltpeter is often more common than sodium chloride; hence, the substance actually added was probably saltpeter, just the ingredient needed to make a pyrotechnic mixture. The proportions were almost certainly not stoichiometric. The burning rate was therefore probably fairly slow, slow enough to make the composition better suited for use in rockets than in guns.

This succession of fortuities was crowned by a modification to the arrow itself. As every Boy Scout knows, it is easier to kindle and nurture a flame if it is shielded from the wind; hence, it is likely that the military engineers enclosed the front and sides of their pyrotechnic mixture, perhaps with a joint of bamboo. Thus the first rocket motor was born. These early craftsmen probably did not realize what they had accomplished until an observant archer noticed that the new fire arrows flew with greater speed than the original ones. Sooner or later one of these new arrows must have left the bow unassisted, much to the astonishment of everyone who saw it.

Early Powder Rockets

The Chin Tartars were evidently the first to actually use these fire-arrow rockets. The occasion was the defense of their temporary capital against the Mongol hordes of Ogdai, son of Ghengis Khan. The siege took place in A.D. 1232 at Kaifeng, the capital of the Chinese province of Honan. The city fell, and one can well imagine that the Mongols went to special pains to learn the secret of this terrifying new weapon. Apparently they succeeded, for when Bagdad was attacked by the Mongols in 1258, similar equipment was used. The defenders of this unhappy city may have been fortunate enough to come into possession of some of the new devices and perhaps even of some of the armorers who made them. At any rate, they soon solved the riddle of the secret weapons. An Arabian manuscript prepared by Hassan Alrammah in 1280 gave formulas for making gunpowder and instructions for building rockets, which were called Chinese arrows.

Knowledge of these things had also traveled more or less simultaneously to Europe. Rockets and rocket powders were mentioned in the *Liber Ignium,* which was authored or translated by Marcus Graecus in the first half of the thirteenth century. The chronicle of Cologne on the Rhine spoke of rockets in the year 1258. The rocket proved to be the decisive weapon in the battle for the Isle of Chiozza in 1379. A defending tower was not overcome until it was set afire by a rocket.

Several variations of rockets existed at the beginning of the next century. In his *Bellifortis,* which was completed in 1405, Konrad Kyeser von Eichstädt wrote of floating rockets, rockets traveling along guiding strings, and skyrockets; i.e., rockets that were launched vertically. The string-guided versions were evidently used for communication purposes. Kyeser also was the first to refer to the practice of boring a hole in the center of the powder charge to increase the burning area. This was a fundamental advance in rocket construction. It was the first step toward what is now known as the unrestricted-burning rocket.

Many applications of rockets were given by the Italian engineer Joanes de Fontana in his *Bellicorum Instrumentorum Liber,* which was completed in 1420. Among them was the first design for a rocket car. The drawing showed an exhaust nozzle that may have been a forerunner of the modern De Laval nozzle.

By the beginning of the sixteenth century most of the variations of the powder rocket were known, and designers had begun to turn their attention to auxiliary matters. Rockets equipped with small parachutes were described for the first time in this period. Thought was also given to the important problem of stabilization. The traditional stick was discarded in favor of stabilizing fins for the first time in designs that appeared about the year 1550.

The rocket was but one of the many weapons of war in existence at that time. Guns, which had been known since the early fourteenth century, had rapidly developed to the point where they were more than a match for the rockets of 1550. As a result rockets were gradually superseded as weapons except in naval warfare, where they continued to be used as a means for setting fire to riggings. A Prussian colonel named Geissler attempted to use the rocket for a similar purpose in land warfare. He experimented with rockets that weighed one hundred and twenty pounds and carried a slow-burning incendiary bomb. The tests, which took place near Berlin in 1668, were discontinued when it was decided that the rockets were unsafe.

Display Rockets

Although the rocket fell into disuse as a weapon, it did not disappear from the scene entirely. On the

contrary, it began to achieve a wide popularity as a means of furnishing recreation. The firing of rockets and other pyrotechnics was a feature of almost every major celebration in the seventeenth and eighteenth centuries. The displays conducted by the Italian school became famous throughout the world during this period. The Ruggieri family of Bologna traveled to many European capitals, bringing with them not only brilliant displays but also the term *rocchetta*. This is a diminutive form of the Italian word for distaff, an object that resembles the stick rocket in general appearance.

Rockets which once were used to gain victory were used even more lavishly to celebrate it. In 1749 the Peace of Aix-la-Chapelle was marked by pyrotechnics in London, Paris, the Hague, and Dublin. King George II invited Gaetano Ruggieri to conduct the London display, which was described in a special book published by the Board of Ordnance. The backdrop, in the form of an elaborately decorated Doric temple, was four hundred and ten feet long, one hundred and fourteen feet high and took almost half a year to build. More than ten thousand rockets and twenty thousand pyrotechnic devices of other types were fired in a celebration that lasted more than six hours. Enough fireworks were left over from this affair to provide for another large display on the Thames a few weeks later.

This period of festivity was not without fruit from the rocket research standpoint. The step rocket was evidently first thought of at the end of the seventeenth century. It consisted actually of two rockets mounted and operated in tandem. The after, or lower, rocket was fired first and, when it was spent, the forward rocket was ignited. The purpose then was to make a more spectacular display; today the same method is used to gain greater altitude.

While the Europeans were engaged in increasing the size and splendor of their rocket displays, an Indian, Prince Hydar Ali Cawn of Mysore, was busy trying to reapply the rocket to warfare. His efforts were not without success, for he used rockets with good effect against the British in fighting along the Coromandel Coast from 1780 to 1784. His army boasted a rocket corps of more than a thousand men. The rockets were similar to the ones used in England for celebrations. They were much larger, however, weighing from six to twelve pounds. Their bamboo guiding sticks were ten feet long. The cardboard case used in display rockets had been replaced by an iron one. These rockets had a range of about twenty-five hundred yards. They were not too accurate, but they were very effective when fired in large numbers against the British cavalry.

The rocket corps was soon expanded to five thousand men, and the rockets themselves were improved by the addition of an explosive warhead. The British met these improved weapons at Seringapatam in 1799. The explosion of one of the rocket warheads killed three of their soldiers and wounded four others.

The Congreve Period

All this was not without its effect in England. It is a matter of history that those who suffer at the hands of a new weapon are stimulated to imitate it, and to improve upon it. Colonel William Congreve took up the challenge, and it is his name that is associated with this era of rocket research. Rocket development had been progressing in India for a quarter of a century when Congreve began his experiments in 1801. His contribution was to increase the size, variety, and effectiveness of rockets, and to put them in the hands of a European military establishment. Their use by the British in many of the major engagements of the time resulted in their being known, copied, and studied by all the leading nations.

Congreve began by investing his own money in the largest display rockets then available. He determined that the range of these missiles was about five hundred yards. With the aid of his father, who was comptroller of the Royal Laboratory at Woolwich, Congreve obtained permission to use that laboratory's facilities. He soon increased the range of his rockets to about two thousand yards, and in 1805 he was able to conduct a successful demonstration for the Prince Regent.

Characteristically, the British first made use of these rockets as a naval weapon. The target was Boulogne, one of the points at which Napoleon had gathered his forces for a cross-Channel invasion of Great Britain. The attacks made in the fall of 1805 were ineffective. A second attempt, made a year later, was much more successful. Eighteen small craft were able to fire two hundred rockets in salvos in about thirty minutes. The range was a little less than one thousand yards. The fires set by the rockets destroyed a considerable part of the city and its concentration of military equipment.

The success of this raid evidently spurred the Royal Navy to make large-scale preparations, for by the next year it was able to launch more than a hundred times as many rockets at Copenhagen. From twenty thousand to forty thousand were fired at the relatively inflammable structures that existed in that day. The resulting fires may have reached what is now referred to as the saturation level, for most of the city was destroyed. This epi-

sode established the Congreve rocket as a force to be reckoned with. The British Army also took up the new weapon. Rockets helped to defeat Napoleon at the crucial battle of Leipzig in 1813, and they were used against him in most of the engagements that followed.

It would be hard to imagine a more dramatic set of circumstances for the introduction of a new weapon. The blows which Congreve rockets struck at Napoleon as he bestrode Europe aroused a tremendous interest. Rockets were quickly adopted by almost every country on the Continent. They were used not only by the home forces but also in overseas and colonial warfare. The Dutch used rockets against natives in the East Indies. The Russians employed them in the frequent skirmishes with the peoples who lived at their Siberian borders. British forces used rockets in Africa.

Congreve rockets even found their way into the Western Hemisphere. They were used as a surprise weapon in the battle for Washington that was fought at Bladensburg, Maryland, in 1814. They evidently had the intended effect, for the British won the battle and entered the capital. The event is now remembered chiefly for the fact that the Presidential mansion was burned and later was given a coat of white paint to hide its scars. It was less than a month after the burning of Washington that the celebrated battle for Baltimore's Fort McHenry took place. The rockets immortalized by Francis Scott Key in *The Star Spangled Banner* were Congreve rockets. The American Army was not immune to the rocket fever, and in time the new weapons were made part of its battle equipment.

This widespread interest in rockets was naturally accompanied by a great deal of research activity. Congreve had continued his own efforts toward improvement. Noticing that the side-mounted guiding stick impaired accuracy, he moved it to the axis of the rocket and reduced its length from twenty-five to fifteen feet. This was made possible by fitting the rocket with a base plate. The guiding stick screwed into the center of the plate, and the gases escaped through four conical holes around its periphery. In some models a fork with three or four prongs was used to attach the stick to the rear of the rocket. Congreve developed a wide variety of rocket types, ranging in size from twelve to forty-two pounds. The larger rockets each carried a twelve-pound bomb and had a range of three thousand yards.

In 1827, the year after Congreve's death, Dutch experimenters constructed a stickless rocket stabilized by three vanes. Apparently it was not very successful. The French established a rocket research commission, which developed a forty-pound missile having a range of three thousand five hundred yards. The armies of Denmark, Sweden, and Poland also conducted rocket research. The subject was so shrouded in military secrecy, however, that almost nothing is known of the Scandinavian work, and the only Polish report available is one written before the rockets were tried.

An American named William Hale made an important contribution to rocket research in 1846. He discovered an effective alternative to the stabilizing stick. Hale installed three curved vanes in the orifice of the rocket motor. The jet, acting on these vanes, caused the rocket to spin, and the gyroscopic action provided the necessary stabilization. This principle is still in use today. Hale rockets were adopted by the American Army, by the British for colonial warfare, and by the Austrians for use in the Alps.

In spite of these improvements, rockets were soon to be far out-performed by guns. Congreve's rockets were more than a match for the smooth-bore ordnance of his day. The principle of gyroscopic stabilization, however, was applicable to shells as well as to rockets. Rifling greatly increased the accuracy of gunfire. Chemistry provided the powerful new smokeless powders which progress in metallurgy made it possible to exploit adequately. These advances, which occurred in the latter part of the nineteenth century, had an enormous effect upon the performance of artillery. Improvements to rockets in this period were in no way comparable. Thus, for the second time the war rocket was eclipsed by the gun.

Rockets meanwhile had begun to play quite another role, that of saving lives. They were used to run lines to vessels foundering in coastal waters. The early attempts were often handicapped, however, by the limited range of the rockets. In 1855 Colonel R. A. Boxer developed a rocket of much greater range by employing the step principle. His work was done at the Royal Laboratory, Woolwich, where Congreve had experimented half a century before. Boxer's two-stage, line-carrying rocket was put into service at coastal stations, where it provided the crucial increment of range in many rescue efforts. In the years that followed, line-carrying rockets saved more than twelve thousand lives in English waters alone.

During the nineteenth century the general interest in the problem of flight led to several suggestions for the use of rocket propulsion as a means for powering aircraft. Most of these schemes envisioned the use of steam jets or of an apparatus

similar to a swiveled machine gun firing blank charges. P. E. Paulet, a Peruvian engineer, carried on a series of experiments with a small pulse-jet combustion engine from 1895 to 1897.[8] His motor, which was fueled with nitrogen peroxide and gasoline, developed a thrust of about two hundred pounds. It was apparently the first reaction engine to rely entirely upon liquid fuels. Paulet went no further, however, and it was not until thirty years later that he published the account through which his work became generally known.

FIG. 3. Goddard rocket shortly after it left launching tower (August 26, 1937). This liquid-fuel rocket was more than 18 feet long, or approximately the length of today's Wac Corporal. It was steered by means of a gyroscopic control similar in principle to that used in the Navy's Viking. (Reproduced by permission from *Rocket Development,* by R. H. Goddard, Prentice-Hall, New York, 1948.)

The rockets in use at the end of the nineteenth century were similar in many important ways to those developed hundreds of years earlier. They were driven by powder fuels and, judged by present-day standards, they were not very efficient devices. The dramatic developments that have become synonymous with rocket research did not begin to unfold until the first half of the twentieth century.

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THE TWINS

Wrapped in the mantle of the mind which draws
Its conscious, curving lines from point to point,
Mathematics, with her eyes upon the stars,
Stands firm upon the vantage ground of truth.
Beside her Poetry, firm hand in hand,
Seeks beauty and invention and delight
In perfect symmetries of sense and soul,
Which this as yet unfallen speech defines.
Twin sisters these yet not identical,
Immaculate conceptions of clear thought,
Distinct and beautiful, precise in form,
Mathematics musical and poetry
Exact, each striving for the infinite
Expressed in symbols as best suits its case,
One for the head, the other for the heart.

HELEN ROWE HENZE

A Planned Economy: Good or Bad?

RUTH SHALLCROSS

Economist at The Institute of Paper Chemistry since 1946, Dr. Shallcross was Chief of Program and Statistics, War Production Board, during World War II. Her graduate work was done at Bryn Mawr and was supplemented by work at Columbia and studies in London, Paris, and Frankfort. She has published a number of articles on the economics of the pulp and paper industry. Her present discussion was evoked by the article by Elgin Williams that appeared in THE SCIENTIFIC MONTHLY *for February 1950.*

BARBARA WOOTTON, one of the best-known British economic writers on "planned economy," attributes the original interest in the term to the widespread attention attracted by the Soviet Five-Year Plans.[1] Coming, as they did, before and during the most severe economic depression this country had ever witnessed, these plans seemed logical and made individual economic planning seem particularly chaotic. Moreover, with the growing emphasis on technology and on the scientific method, the very word "planning" carried with it a connotation that precluded opposition (and unfortunately analytical thinking) to what seemed to be a necessary reorganization of society along rational lines.

A nation's economy in the modern world is organized either (1) on the fundamental principle of individual determination of economic values in a free market wherein decisions are freely made by millions of producers and consumers and reflected in the prices resulting therefrom, or (2) on the principle that the government (through administrative agencies, economic planning boards, *et al.*) should determine values by various types of state controls and plans.

The thirties witnessed a tremendous impetus to the idea of a state-directed economy. Many who accepted the concept under the name of "planned economy" (or "a welfare state") have failed to recognize its inherent socialist philosophy.[2] Like socialists before them, those advocating a state-planned economy have attacked profits in the free economy because they seem to epitomize the selfish individual acting independently to further his own good at the expense of the public's welfare. For the most part, the new world planners, also like the socialists before them, have accepted in principle, if not as stated, Marx's dictum,[3] "From each according to his abilities, to each according to his needs," or, as the American Socialists phrased it, "production for use and not for profit"—both concepts implying a centralized economic authority that would determine "need" or "use;" in essence, a state-planned (or controlled) economy.

Few remembered that for a long period of time —one hundred and fifty years—the American public had in fact fared very well with a free market economy now said to be chaotic, in which individuals were allowed to seek their own profit with a minimum of governmental regulation; and, as Adam Smith predicted, great wealth and prosperity for all the people had in fact resulted. The record of improved welfare is clear for all to see. Except for the 1930s, each decade had witnessed more people producing a greater volume (and better quality) of goods per employee than the previous one. Over the past fifty years, the standard of living for all had improved miraculously with a decline of more than one third in the number of working hours per week, with real purchasing power per hour increasing threefold.[4] Joseph Schumpeter states that "if capitalism repeated its past performance for another half century starting with 1928, this would do away with anything that according to present standards could be called poverty, even in the lowest strata of population, pathological cases alone excepted."[5]

But the fear of loss of property and income, together with evidence of actual loss for a segment of the population during the thirties, clouded the sights of many to this impressive record and to the fact that, even during that depressed period, per capita production was "high relative to the best years of many other countries and to the best years of our not-too-distant past."[6] Even the "government planning" during that era and since, which has created more problems (especially deficit financing) than it has solved, has not dispelled the

strong feeling, born of the depression, that the government should guarantee certain economic rights, even at the expense of transforming the economy and eliminating economic freedom. And, moreover, this attitude seems to prevail despite a new critical attitude toward collectivist economic planning in the Soviet Union brought about by the postwar belligerent and openly imperialistic Soviet policy.

Two reasons may be given for this seeming paradox. One is the insecurity caused by the last war and the fear of another. The second is that many government officials have found it politically expedient to promise all sorts of things that could only be delivered, even in a small degree, by having complete power over the economy. Those who are asking for state power to control the economy (or, as they say, to establish the welfare state) are doing so paradoxically "in the interests of saving free enterprise." But why should the state not have complete control over the economy?

Present-day skeptics of a planned economy, or the welfare state, have realized that the most serious problems of economic organization have not even been recognized by those seeking control over the economy, let alone successfully solved. Chief among these is the seemingly simple task of defining and measuring the economic goal of society—the greatest good for the greatest number—in terms that are universally acceptable.

Unfortunately, economics does not have precise yardsticks by which its goals can be accurately measured. But in a free market economy, individual planners (producers) have a very real (even though not precise in a physicist's concept) measure for determining what individuals believe their welfare to be. The measure is prices, which, when uncontrolled, become the common denominator of conflicting ideas of welfare shown in consumers' choice of purchases—the only universally acceptable measure available. But too few understand the intricate working of the price system sufficiently to realize that it is as important a measure in determining the highest economic goal as instrumentation is in determining scientific goals. If any one of the ten million private planners does not promote consumers' welfare, he will be priced out of business. His efficiency, or lack of it, in serving the public shows up as profit or loss. Thus the incentive to promote the public's interests is high. The growth of marketing research substantiates the growing desire on the part of producers to measure consumers' choice more accurately. Imperfect judgments, of course, are to be found in a free market economy as in a controlled one, but the responsibility for economic decisions in a free economy is spread among many enterprising producers and, hence, is not as great as that of a centralized planning board. Moreover, penalties for errors in judgment are restricted to a few in a free economy but affect the entire economy in a centralized planned system.

State economic planners would throw out all the aids to planning that the market economy gives to private enterprisers. They have no magical power of measuring whether, for example, new machines should go into one company or one industry instead of another, but must fumble, and guess, and arbitrarily and authoritatively make a decision. Prices, like a thermostat, are of no value as a measure of economic activity if they are controlled. Profits, if eliminated, cannot determine efficiency of production operations. Would the state planners eliminate losses which in a free market system are as effective a measure of inefficiency of production as profits are for measuring efficiency—both being prices (either positive or negative) paid for enterprising activity in risk-taking? Are there to be tests for the planners' efficiency? Or are the planners assumed to be supermen without error in judgment? These are important questions which those in favor of a state-controlled or "planned economy" dislike to discuss. They usually state that the details will be worked out after power has been achieved or they speak vaguely of helping the needy producers (in practice, the most politically expedient needy); but this usually means giving aid to inefficient producers, who, from the public welfare point of view, ought to fail.

Oskar Lange, a socialist economist formerly at the University of Chicago (now purported to be unhappy in a Polish university), has advocated that the state (in a planned economy) should simulate the free market criterion of economic welfare.[7] But that would be impossible without actually having a free market—a tacit admission that a free market is preferable to any artificially controlled economy.

Sir Henry Clay, in speaking of the economic controls of the British Labour Government, presents the problem of consumer welfare where state planning is not complete in all branches of the economy:[8]

> The Ministry pays what price it must to secure supplies; being relieved of the necessity of covering its costs out of its receipts, it sells at prices fixed arbitrarily, often in relation to conditions that no longer obtain. The consumer buys, not what he would choose if he had to pay the true cost, but what he chooses at the price (less than the cost) which the Ministry thinks he ought to pay.

"Nationalization," he goes on to say, "has deprived the consumer of such protection as competition, actual or potential, offers." Maurice Dobb admits

that socialism can be achieved only at the price of abandoning the economic freedom of consumers.[9] But socialists or state economic planners would have few followers if they were as realistic.

Half-way planning schemes usually lead to more and more government plans unless the people are constantly aware of and vigorously oppose them. Therefore, a nation can be led step by step, unwittingly, into a full state-controlled economy. This is the method of bringing about socialism deliberately adopted in the nineteenth century by the British Fabians, now influential in the Labour Government, and it is the method most successful in this country. Many who have accepted the goal of a state-planned economy become very emotional over the means to that end. The Fabians have won a large number of adherents to their mild evolutionary methods as distinct from the crude revolutionary and disruptive methods of the communists. However, it is the goal itself that needs to be questioned.

Because of lack of space to develop the myriad facets of a state-planned economy versus a free market economy, the following six points are discussed briefly to show some of the drawbacks of a government-controlled system.

First, a centralized planned economy cannot evaluate individual welfare in terms that are universally acceptable, as was elaborated earlier.[10]

Second, when a centralized plan has been adopted it becomes important that it be carried out irrespective of the opposition from the public and, thus, is inflexible and insensitive to the public's desires. This psychology is evident in discussions with most advocates of a government-planned economy who believe that the public should be forced into accepting an economy that will be "for its own good." Public welfare is defined by planners in power as something they think is good for the people. The various groups of "planners" argue heatedly over whose blueprint of a new social order will be accepted and forced onto the people. Thus it is evident that most collectivist economic planning is authoritarian in thinking; it assumes that the public is incapable of determining its own welfare and must have superior planners do it for them. It is totally at variance with the democratic ideal which presupposes that people have the right to direct their own welfare and to be free—even to be foolish if they so choose.

Third, planned economies have not proved that they are even as stable as a free market economy, which has been accused of instability. In this regard John Jewkes, of the University of Manchester, comments on the power crisis in England in 1947:[11]

> No country has ever suffered from a more sudden or catastrophic economic seizure. Unemployment rose temporarily to over 2,000,000. The crisis probably lost Great Britain £200 millions of exports. The overall loss of production cannot be estimated with any precision. It is sufficient to say that Great Britain had suffered a crippling blow, the effects of which were largely instrumental in precipitating the balance of payments crisis suffered later in the year.

Erik Lindahl admits that the Swedish monetary system (which he regards as part of the national planning program) has "an inherent instability which can be regarded as a characteristic feature in comparison with the previously existing more automatic systems."[12] Ralph Holben, in discussing state planning in Norway, shows that a planned economy may "substitute one form of instability (that of the balance of payments) for another form of instability (that of domestic prices),"[13] with the former being especially serious for a small country. He concludes that "one should not be deceived into thinking that once the magic wand of economic planning is waved, the problem of economic stability somehow disappears."

Fourth, a centralized planned economy leads to international conflicts. In peacetime, there is no economic goal higher than individual welfare as determined by the people themselves; during a state of war, consumers are willing to let their own interests be subordinated to the nation's war efforts and will accept, temporarily, whatever goods are offered, shoddy or otherwise. The public is convinced sacrifices are necessary when a real national emergency exists. Because leaders of a planned economy cannot give the people the welfare that they could obtain for themselves in a free market economy, the planners must continue to foment international emergencies. Thus, imperialism and international unrest are far greater in evidence in a strictly controlled economy than when free trade is allowed between private commercial traders who, with the exception of armament makers, have nothing to gain and much to lose from international conflicts. The type of power that is concentrated in the hands of the few state controllers or planners of the economy leads them to use that power for world conquest and more power, as Russia has so very well demonstrated.

Fifth, those who would substitute a government-controlled economy for a market system indict the latter with monopolistic practices which places controls in the hands of a few business leaders.* Yet

* Of interest here is the study of Clair Wilcox in which he concludes that "oligopoly [few producers] is not, by all evidence, the ruling market form in the modern economy and that it does not, in fact, comprehend the great majority of actual cases."[14]

the state-controlled economy would set up a complete state monopoly over all the economy, with all the evils of private concentration of power compounded. Private monopolies without state sanction have in the past been short-lived and frequently have been broken up by the introduction of a substitute product, but this situation could not exist under a planned economy where all industries are nationalized, and, hence, monopolies.

A state-planned economy has been admitted by socialists to be incompatible with free consumers' choice. Thus the last and *sixth* point is that the economic freedom possible only in a free market economy is basic to all other freedoms we in a democracy hold dear. Much evidence exists to show that state economic planning and individual freedom are completely incompatible, the opposition of some spokesmen of economic planning to the contrary notwithstanding. If the state is to take from each according to his abilities (whether by out-and-out confiscation, devaluation of the purchasing power of the dollar, or by progressively confiscatory taxation) and give away the wealth of that production in accordance with need or use (as the welfare planners advocate), someone must arbitrarily and authoritatively determine who shall produce what and where and how much and who is in need. There is no other alternative. Many a Britisher is finally realizing that the application of the idealistic Fabian phrases are far different in practice than in theory. To be effective, official planning of the economy cannot tolerate those who differ with their plans; thus, free speech is certain to be jeopardized, as Russian planning has demonstrated. The more effective the economic planner, the more dictatorial he (or the planning board) must be and the more the government takes on the characteristics of a complete police state, allowing little freedom for the people.

History is full of the struggles of people seeking freedom from state power. State tyranny has always masqueraded under the guise of promoting the public welfare. Lord Acton has warned us that "power corrupts and absolute power corrupts absolutely;" yet we blithely give the state more and more power over our lives with the vague hope that the officials having that power will, contrary to all historical evidence, use it wisely and in accordance with their promises of welfare and security.

It may well be that the average man really wants the kind of an economy which is completely and authoritatively planned for him, just as many Southern Negroes preferred the slave system to the freedom offered them after the Civil War, because they preferred not to take the responsibility for their own economic welfare. During any postwar period one is tempted by political promises of security. But it is doubtful indeed that the majority of the people want a completely planned and controlled economy.

New debates need to be held by informed public leaders on the role modern government can play in an economy of a free people which would in truth promote their greatest welfare. From these debates, fundamental principles need to be reformulated, as in the days of the Federalist writings, and then maintained with moral conviction. Unless fundamental economic and political principles are held with moral conviction by the majority of the people, they are apt to be lured by false promises or forced into authoritarian schemes by pressure from those who crave power and who would seize it by pretenses that the public welfare makes it necessary.

As the United States stands on the verge of having an economy that is being more and more controlled by the state, or of retaining a free market economy in which the welfare of the people is determined by them through their influence on prices, one might well ponder the statement made by Professor Jewkes:[15]

> For central planning ultimately turns every individual into a cipher and every economic decision into blind fumbling, destroys the incentives through which economic progress arises, renders the economic system as unstable as the whims of the few who ultimately control it and creates a system of wirepulling and privileges in which economic justice ceases to have any meaning.

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The Problem of a Universal Language

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THE desire for a world language, felt in many quarters over the past few centuries, has assumed new urgency because of the present closeness of international relations and the setting up of various international bodies, from the United Nations down. On the other hand, the variety of solutions already proposed has produced a conflict of opinions that has not so far helped to put the problem in a clearer light. It is worth while to consider some of the fundamental aspects of that problem before any choice among proposed solutions can profitably be made.

The necessary point of departure is the obvious fact that variety is and always has been a main trait of human language. The distinguishable sounds that the vocal organs can make are not too numerous; but they have been combined into an amazing number of languages, distinct in both sounds and structures, and endowed with remarkable powers of survival. This statement, true today, is also true as far back as our historical knowledge extends. To this is added the tendency of each well-developed language to split into dialects, which often diverge so far as to become almost unintelligible to speakers from different parts of the same country. The result may be depressing to those who dream of a unified humanity; but nothing is gained by trying to ignore either the differences that exist or the tenacity with which they are maintained. There is no evidence that mankind in general has ever been gravely concerned over this condition; it has been accepted as annoying, perhaps, but as natural, and the efforts to remedy it have been practical and more or less obvious.

The territory over which a given language is spoken seems largely dependent on chance. The spread of what we call the major languages has not been due to any abstract discussion of their respective merits, but to the enterprise of those who used them. Some of them have expanded widely in the wake of mercantile and territorial expansion, others have remained within restricted limits; but most of them have found defenders of their virtues, and national pride has backed up the mere effect of familiarity on their users. It is therefore unlikely that any general arguments will truly convince the users of a language that they should use another; the compelling reasons must be immediate and practical if they are to carry weight. The inhabitants of neighboring countries may find it necessary or convenient to know something of each other's tongues; but they will not do so on a large scale until fairly extensive relations have been established, and the amount of such contacts will fluctuate with changes in those relations. The present language map of the world is the result of centuries of such fluctuations. We may or may not like it, but we have to accept it as a fact of the world as it is before we can think of changing it.

Moreover, this linguistic map has suffered drastic changes in the past five years. In 1945 the presumed trend to "one world" appeared to favor the free exchange of information, the breaking down of national barriers, and an emphasis on all forms of international organization. Today the spread of Communism has split apart large portions of the world, and given Russian an unexpected linguistic advantage. China, which had been developing contacts under American influence, has been sharply reoriented, and eastern Europe is increasingly under Russian influence, with a corresponding fall in the prestige of Western languages. In more restricted fields, national feeling helps to keep differences alive; thus Ireland is renewing the attempt to revive Irish as the national tongue, and Puerto Rico is reverting to the official use of Spanish. Whether or not linguistic diversity ought to be diminished, it is at the moment clearly on the increase.

Taking the situation as it is, and not as it might be, we see that every human being has a mother tongue which he can speak more or less competently; but by no means every human being can read and write that tongue, though he may also speak another language than his own. It may be held that everyone should be able to read and write; but that end, desirable as it may be, is not identical with the problem of establishing a new tongue, though many writers seem to assume that it is. The notion of general literacy is perfectly compatible with the existence of the present diversity; but the general adoption of an auxiliary language presupposes a fairly general literacy that in many places does not at present exist. It would make for clearness if these aims were kept distinct, as in many connections they must be if the situation is to be properly understood; but for our present purpose it is enough to take for granted the level of literacy which now prevails, and not to raise the question of what a wider educational policy would involve.

Why We Learn Foreign Languages

Coming to the main matter, why does anyone learn a foreign language? At least four main motives can easily be distinguished. One is for ordinary intercourse, business or social, usually by speech; a second is for engaging in diplomacy or directives, which may involve both speaking and writing; a third is for gaining scientific or technical information, largely from written sources; and a fourth is for what may broadly be called cultural purposes, which again will deal mostly with written language. It is evident that each of these motives may be pursued by itself, as well as in combination, and that each has its own requirements, which may differ considerably. A person who can speak a language well will presumably be able to read it; but the converse need not be true. Again, the ability to speak a language for immediate practical purposes need not mean an ability to use it extensively or elegantly. There is room here for an indefinite variety of adaptations. What each person would like to get, and what he is able to get, in the way of linguistic skill will depend on many considerations, and there is no standard prescription that will take care of all cases.

For ordinary purposes of communication, the difficulties that arise are got over by the learning of a new language, by the use of interpreters and letter writers, and by the adoption of simplified common languages such as pidgin English and Chinook. Seaports and seafaring populations are naturally favorable to the growth of such devices; but they may also be found in inland areas where various nationalities come into contact. Thus, in northern India and the adjacent lands, a person with a knack for languages may pick up several, speaking such combinations as Tibetan, Nepali, and Urdu, or Urdu, Turki, and Persian, with lesser-known tongues or dialects thrown in. On the other hand, dwellers in a homogeneous inland area may have no contact with other nationalities, and no occasion for the use of anything but their native tongue, unless, as often happens, a foreign language is employed for certain religious purposes. The only way to discover what is true in a given area is to examine it on the spot and see what languages are in use, and how widely.

In a world that abounds in "directives" and consultative bodies, the language of governmental and diplomatic action assumes fresh importance. Here we have, on the one hand, the question of making orders intelligible to those who live under a particular government, and on the other that of establishing easy communication between governments. Under the former head, we have both the older colonial systems, so far as they may survive, and the newer projects for furnishing technical assistance to undeveloped areas. A colonial power will naturally extend its own language to the country it rules, and traces of that relation may well remain after the original system has departed; India will keep English as the common official language, and adjustments will have to be worked out in each case, just as they were when explorers and traders first arrived in a given territory; many of the old devices will doubtless still be found workable.

In the field of diplomacy we have again two possibilities—one, the use of a general language in official proclamations and treaties, or in the open debates of the United Nations; the other, the language adopted in private diplomatic conference. The former is at present covered by the use of several official languages, and by the increased use of interpreters, a system which, though cumbersome, appears to be working as well as could be expected. It is unlikely that mature statesmen will devote much of their time to increasing their linguistic equipment; they will more probably use what they have and rely on assistance where needed. As for private diplomatic conferences, they will doubtless be carried on, as in the past, by whatever means are most convenient, varying according to the particular circumstances of the nations concerned. These variations, as already indicated, are likely to increase with present changes in political alignments; but they will probably not be altered by any general device imported from outside.

In the field of technical publication a similar

duality confronts us, that between work intended for specialists and that intended for the general public, to acquaint it with the main results of specialized research. Much of the former may be conveyed in mathematical terms, with a minimum of accompanying explanation; the latter verges on, or may pass into, a purely literary treatment, in which technical terms are reduced to a minimum, or carefully explained. The range between these extremes is very wide, and the problem of satisfying all requirements is not easy to solve. It is often said that the presentation of scientific results to the general public in assimilable form is an important task for our age, and so it is; but the best way of doing it is perhaps yet to be found. In any case, it will not be identical with the use of a group of already settled symbols, verbal or abstract, surrounded by a minimum of explanatory tissue, especially as we see that this tissue is by no means everywhere the same. The supposed universality of science is quite compatible with the use, by each scientist, of his own tongue, managed with whatever skill he may command.

Something has been done to reduce the multiplicity of technical articles by the regular publication of abstracts; but only certain fields are covered in this way, and there can never be complete certainty that a very brief abstract does justice to its original; it may at best be a guidepost, and resort to the original may be needed for serious study. Most specialists presumably acquire sufficient linguistic equipment to find what they want; but that equipment must again vary with the individual case. As for translations, they may or may not be available; in any case, they cannot be made to cover the entire field of knowledge, and it is largely a question of chance whether even an important work gets translated or not. The efforts to improve and extend translation by the help of international organizations are still too young to have made much impression, or to indicate just how far they will be able to go; and since not everything can be translated, and one person's choice of what should be selected may not be another's, the outlook for substantial improvement is not too bright.

As for the study of languages for cultural purposes, that will obviously proceed according to personal preference; any language may be studied on its own merits by anyone who thinks it worth while to do so. The greater languages will naturally attract more attention than the lesser, and the learner is more likely to be drawn to tongues akin to his own; but any language may attract a student who thinks he has sufficient grounds for acquiring it. Such a pursuit, if seriously undertaken, differs in its motivation in that it brings the student, at least in principle, into contact with the entire word-stock of the chosen tongue, not, as in the other cases, with a portion determined largely by practical considerations. So long as written records exist, so long will this type of interest find its followers; but it does not directly affect the status of most languages, except insofar as it supports their prestige in a particular territory or class.

Requirements of a Universal Language

Such, then, is a cursory review of the chief aspects of our present linguistic variety, of the conditions that have led to the desire for a universal language as a corrective or a supplement. But it is clear that every living language has been large enough to meet all these needs by selection from it, and a satisfactory new language can offer no less. Such a language, it is obvious, will not be easy to get. Too many advocates have been content to point out that it would be desirable, that if books could everywhere be read as easily as musical notations or tables of logarithms, a vast amount of time and effort would be saved. No doubt they would; so they would, if we did not have to take time to eat and sleep. Unfortunately, neither dream is realizable. From the abstractions of number, or the twelve notes of the ordinary musical scale, it is an enormous distance to a fully developed language, and the attempted parallel is meaningless. Any proposed language must be prepared to do what existing languages are able to do, or else be reduced to the status of a mere subordinate device that is not properly a language at all.

It is worth noting that the first proposals for a universal language originated with philosophers like Descartes and Leibnitz, who were primarily concerned with the logical ordering of primary concepts for the benefit of scholars, rather than with a vehicle for general intercourse. This logical trend has long dominated the subject, and has led to the desire for a language marked by phonetic spelling, regularity of forms and syntax, and absence of idioms. In fact, however, no language known to man has ever perfectly displayed these traits, which suggests that after all they may not be indispensable, and that too much emphasis on them may actually be a hindrance. Nonetheless, at least five major attempts on this model, along with numerous variations, have been offered in recent years, each proposal provoking a new one, or at least a number of criticisms, and no single one finding general acceptance, or being able to prevail decisively over its rivals. It seems that the principle

of linguistic variety besets even the attempts to diminish it or to get rid of it.

There are only two sources for the words that are to make up a new language: arbitrarily invented sounds, or sounds taken from existing languages, with the idea of making them cover meanings common to as many languages as possible. The former has little to commend it, the results being so unlike any known language as to be repellent; but the latter has to face the difficulty that not many sounds have similar meanings even in closely related languages. This has meant a resort to Latin forms more or less modified, to some Greek forms, or to forms found to be largely common to either Romance or Germanic languages. The reduction of these varied ingredients to relative uniformity has not been easy, especially as some of the results are clearer to a student with a historical background than to an ordinary user; and the impression produced by the various borrowings may seem to be rather a distorted copy of one or more existing tongues than something which can exist in its own right. It can be argued that some of these difficulties will tend to disappear as the new language is actually used; but this is really to expose it to the same sort of competition that it was originally intended to avoid, and to endanger the ideal of prompt and widespread utility.

While the proponents of new languages have been urging the claims of their respective creations, another solution has been proposed, that of simplifying some existing tongue. The liveliest proposal here has been that of Basic English, already old enough to have attracted a wide following, and also to have raised the inevitable seeds of variety. At first sight, it might seem that a selection of words based on frequency of occurrence, and supported by a minimum of grammatical structure, would be well suited for general intercourse, and could be widely diffused in a relatively short time. However, the advocates of Basic have not made it clear whether it is to be a purely auxiliary language or whether, as the "translations" of existing works suggest, it is to be promoted more or less in its own right. It is quite possible to admit the first of these claims without admitting the second, and it is important to keep them apart, since time has shown that certain points about Basic are decidedly open to objection.

The chief of these points is the reduction of all verbs to a small number of "operators," which are made to function in verbal phrases, according to a mistaken parallel with mathematical procedure. It is not possible to treat sentences as if they were equations; even the most technical discourse is hardly prepared to do that, and in ordinary speech it is out of the question. Many Basic phrases are awkward and unnatural; others conflict with similar (often idiomatic!) phrases already in the language. So long as the vigor and color of any language reside largely in single specific words, be they verbs, nouns, or adjectives, it is unlikely that its users will be content, under the stress of strong emotion or need for prompt action, to pause and hunt for their equivalent in a Basic paraphrase. A certain amount of fumbling is inevitable when a new language is being learned; it is quite another thing to make fumbling a basic principle for ordinary intercourse.

Indeed, the whole vocabulary of Basic is more suggestive of counters to be moved about than of words to be used in a living society. When we are told that to distinguish between *thread, twine, cord, string, rope* is merely to heap up names for what is ultimately a difference in size, it is time to stop and consider the differences in the means by which these sizes are produced and the uses to which they are put. It is hardly the same thing to hang by a thread as to hang by a rope; to sew with twine is not the same thing as to sew with thread. If differences of size are not worth observing, it is difficult to see what differences would. So with the stages of growth in time: a puppy is not a young dog, or a kitten a young cat, as those who deal with animals are well aware. In the actual world, we have to deal with very specific differences and changes; does it make sense to throw away the verbal means of specifying these? The abstract language of mathematics has its own place and its own function; but a verbal copy of a few of its features is in a very different position.

Thus it appears that no proposal for a universal language has commanded anything like general assent, or has succeeded in avoiding faults similar to those of existing tongues. Every new claimant makes the choice harder, and if it gains any considerable body of followers only multiplies the variety. Even Basic is admittedly a scheme to be administered by experts, who may have all the limitations of their tribe, as is suggested by the curious nature of some of their proposed additions. Any language is the result of a long and complex growth, and needs to be treated with understanding, not administered by a professedly logical plan which is expected to sweep away anomalies and eccentricities overnight. We have seen in our own time, in the failure of comprehensive efforts to reform English spelling, the danger of attempting too much. It is as unprofitable to offend the dignity of a language as it is to urge improvements before

there is general readiness to accept them. The earnestness, not to say the solemnity, of some reformers does not help to create that readiness. Is there any reason why they should not, on occasion, display a little humility?

In truth, all these reforms disregard the very important factor which Jespersen calls "the desire to play with language." Almost every known tongue has its queer turns of phrase, its verbal oddities, which give it savor and character, and are enjoyed by all save the most helplessly matter-of-fact. Many people like languages, their own or others; they enjoy their own peculiarities, and appreciate those of other tongues. There will, indeed, always be those like Conrad's Jukes in *Typhoon,* "who having no talent for foreign languages mangled the very pidgin-English cruelly," but they are not to be envied. There are also those who cannot make adequate use of their own language; but we do not regard them as setting the standard for what can be done with it. To most persons, there is a pleasure in the neat turn of a phrase, or the initiation into a technical vocabulary, that far outruns the needs of mere rudimentary communication. So long as this feeling of liking exists, it must be taken into account; and liking is not a sentiment which an artificial language is likely to provoke.

It should be clear from all these considerations that the notion of a universal language is not a simple one. It may be taken to mean a language which anyone, if he likes, *can* use; but in the mouths of enthusiasts it tends to mean a language which everyone *will* use. In practice, however, the first of these would more probably mean a language that any qualified person can learn, and the second, one that every qualified person must learn. It is at once apparent that both "qualified" and "learning" raise many questions. It requires a rather robust faith to suppose that every human being is capable of learning a new language, and a very robust one to imagine a system which would make such learning everywhere compulsory. Learning may mean the ability to speak the new language, well or not so well; or to read and write it, but not necessarily to speak it; or to use it as freely and effectively as one would use an existing language. It is hard to believe that all these aims could be realized in a great number of cases and on a world-wide scale; yet that is what the notion of a world-wide language implies.

Recently, however, precisely this aspiration has been put forward by Professor Mario Pei, who in his *Story of Language* maintains that an international language can be selected and imposed by international agreement. The selection would be made by a commission of experts, who would have to reach a majority decision within a limited time. The chosen language would then be put into strictly phonetic form and be taught in the lowest grade of the elementary schools of all countries, side by side with the national tongue, to be learned "easily, naturally, and painlessly" by the oncoming generations. Thus we should "at one stroke do away with all the linguistic difficulties encountered not only by diplomats, but by technicians, scientists, missionaries, immigrants, business men, tourists, and students." This would be the millennium indeed; but if a single decision would really produce all these varied effects, it would be a change unexampled in human history, and less easy to accept the more closely it is considered.

What this plan demands is an amount of international cooperation far beyond anything yet attained, the creation of a world school system far beyond anything yet existing, and a confidence in the ease of bilingualism which is not shared by all students of language. Indeed, Professor Pei is forced to introduce several limitations, such as that the number of delegates to his proposed convention should be determined by literacy, not by total population, and that their decision "would in reality apply only to that portion of any country's population which has the possibility of schooling." Moreover, in his earlier discussion of existing languages, he has admitted that no language has absolute symbol-for-sound correspondence, that there is no royal road or short cut to language learning, and that perhaps phonetic spelling is not the acme of perfection after all. Why should we expect these difficulties to be automatically eliminated by such an international body as we find it hard to imagine, even if a decision were ever reached? Is it likely that national feelings would so suddenly be decreased as to make possible such an educational revolution, even on a restricted scale?

No Language Is Perfect

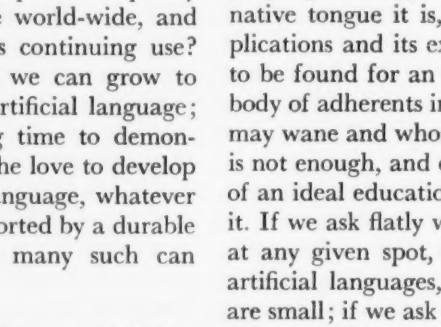
Returning, then, to the linguistic world as it is, we seem forced to the conclusion that no language is perfect, and that the supposed merits of the invented ones are not such as to guarantee their success in competition. It can be argued that what we need is not the search for an unlikely perfection, but increased efforts to improve the means we already have. Every language, no doubt, has its inconsistencies, and even downright absurdities, which can be accounted for, if at all, only by the accidents of historical growth; but it is not feasible to get rid of all these with one stroke. What might be done is to consider how they may be modified

piecemeal in the direction of greater clarity; it has been suggested for English, for instance, that we might in some cases pronounce as we spell, instead of trying to spell in every case as we pronounce. Many existing forces tend to wear down the accuracy and expressiveness of language; it is worth while to consider how they may be resisted without setting up unduly logical and unyielding criteria.

This point of view means the frank acceptance of language as a changing entity, a conception which the advocates of universality seem unwilling to adopt. It has been argued that a universal language would enable its users to meet on a ground of complete equality; but would it? Perhaps, in its early stage, when it was still being learned; but is it conceivable that any language worthy of the name would not come to be used differently by different minds? The notion of a purely "neutral" language is not an easy one to preserve; and if it is to be sterilized by excluding emotional connotations as far as possible, can we hope that a purely intellectual appeal can be made world-wide, and made to serve as a basis for its continuing use? Some enthusiasts maintain that we can grow to love the intrinsic beauty of an artificial language; but the fact will require a long time to demonstrate, and a still longer one for the love to develop on any large scale. The added language, whatever its nature, would have to be supported by a durable and uniform enthusiasm. How many such can history show?

The plain truth seems to be that mankind has no inherent desire for completely logical statement or for world-wide communication; it may be brought to desire them, but only at the price of hard and conscious work. Scientists and scholars will look at them in one way, ordinary citizens in quite a different one; and the needs of any group must be met from a word-stock which is greater than any of them. It is one thing to convey a matter already cleared up, another to clarify a notion still in process of development, and yet another to enforce a proposal against objections; and a different equipment of both thought and language is required in each case. Bacon's ambition to "level all intellects" succeeded neither in his day nor in ours; but the ghost of it still haunts all the advocates of universality. It is easy enough to point out the defects of existing languages, and even to get into a frame of mind which regards mankind as somehow to blame for having evolved them; but the world is still with us, and we have to get along with it and in it.

All world-wide linguistic planning is touched by the fallacy that dogs so many plans in this postwar age—the fallacy of thinking that we can make an entirely new start, and that the new effort will proceed by its own momentum. Every living language is supported by a body of users whose native tongue it is, and who stand behind its applications and its extensions. Where is this support to be found for an artificial language? A scattered body of adherents in many lands, whose enthusiasm may wane and whose descendants may not share it, is not enough, and even the most hopeful blueprint of an ideal educational system is unlikely to sustain it. If we ask flatly what the chances are of finding, at any given spot, a person who uses one of the artificial languages, the answer must be that they are small; if we ask what the chances are of finding that a needed book or article has been translated into one of them, we shall find them still smaller. To realize the force of the competition to which any artificial language is exposed is the first step toward understanding the problem; and it should make every enthusiast at least question the likelihood of any quick and easy solution.



BOOK REVIEWS

THE SCIENTIST AND GOVERNMENT

Report of Special Committee on the Civil Liberties of Scientists. Maurice B. Visscher, Chairman. 63 pp., mimeo. $1.00. American Association for the Advancement of Science, Washington, D. C. 1949.

Science and Foreign Relations. Prepared by Lloyd V. Berkner. vii + 170 pp. International Science Policy Survey Group, U. S. Department of State, Washington, D. C. 1950.

TWO significant documents which have recently been released may well be reviewed together. One is *Science and Foreign Relations,* a State Department report prepared by Dr. Lloyd V. Berkner as a Special Consultant to the Secretary of State and released early in June. The other is the full report of the Special Committee on the Civil Liberties of Scientists of the AAAS. The conclusions and recommendations of this committee, of which Professor Maurice B. Visscher was the chairman, were published in *Science,* August 19, 1949, but the full report has become available much more recently. These reports take up two phases of the currently vital problem of the relation of science and scientists to government. The Berkner report is concerned with activities that may be undertaken by the State Department to stimulate science in this country by facilitating international exchange of scientific information and promoting international cooperation among scientists. The Visscher report is concerned with encroachments that have been made by government on the civil liberties of scientists and the adverse effect of these encroachments on the advancement of science and on the national welfare.

The *Report on the Civil Liberties of Scientists* begins with recognition of the importance of scientific advance to military power and the correlative requirement of precautions to prevent disclosure of some kinds of scientific information. Even if one adopts the purely military viewpoint, forgetting the implications for a vigorous democracy, the imposition of excessive secrecy and the extension of clearance procedures have an adverse effect on the security of the nation. Cutting off all progress in science would give the only absolute assurance that no enemy could benefit from our progress. Elimination of all secrecy in science would make for the greatest progress and the maximum service of science to the nation. The real problem is that of how far we should go in "security" measures that retard American science in order to avoid aid to potential enemies. Where shall we draw the line between security through secrecy and security through achievement? The report concludes that we have gone too far with measures that retard.

In general, not fundamental science but only the technology of its military application should be secret. Although this point of view is widely accepted in principle, the atmosphere created by certain well-known Senators and Congressional committees is such that practice lags far behind. "It is easy to play safe by placing a restrictive classification on a document; it requires clear understanding, close judgment, and self-confidence to release it, at the possible risk of criticism."

Clearance procedures in the AEC and in the National Military Establishment have developed considerably, but there are still fundamental deficiencies. It is recommended that boards rendering adverse judgments should record their opinions, giving findings of fact and providing an analysis of the reasoning that led to the conclusion that the individual should not be trusted. This seems particularly important since much of the "evidence" presented in FBI reports is testimony of unidentified witnesses and since the judgment rendered is an estimate of the future reliability of the individual. Hearings should be provided for AEC applicants who are refused clearance, as well as for present employees. The Industrial Employment Review Board, composed entirely of military officers, passes final judgment on the clearance of civilian scientists employed by industries with military contracts. It reviews the decisions of a personnel officer and gives the individual involved his first and final hearing. There should be an appeal board of civilian composition. A serious practice for the future of American science is the requirement of clearance for scientists doing wholly unrestricted work and having no access to secret data. This is the situation at Brookhaven, where 90 per cent of the work is unclassified. By Congressional action, the practice is now extended to all AEC Fellows. This has an even more serious effect on the securing of scientific personnel when account is taken of the current tendency to avoid recruitment of any scientist who might conceivably encounter clearance difficulties.

The report is highly critical of the "loyalty" program in the Federal government. The only basis for action under the Loyalty Order, not already covered by existing laws or the normal requirement of performance of duties, is "membership in, affiliation with, or sympathetic association with any group designated by the Attorney General as fascist, communist, or subversive." In effect, then, the loyalty program deals only with the way Federal employees supposedly think rather than with their actions. A Supreme Court decision in 1943 points out that "if there is any fixed star in our constitutional constellation, it is that no official, high or petty, can prescribe what shall be orthodox in politics, nationalism, religion, or other matters of opinion." True loyalty in a democracy does not lie in the unquestioning acceptance of governmental decisions, of popular prejudices, or of the *status quo* in political and economic

affairs. It lies rather in thoughtful evaluation and in the participation of responsible citizens in formulation of the patterns of the future. "The Loyalty Order as now formulated and administered runs directly counter to the spirit of the democratic institutions it seeks to foster."

In view of the findings of the civil liberties report, one approaches with trepidation the proposals for extension of the State Department's activities in regulating the international exchange of scientific information. The Berkner report proposes the appointment of a high-ranking scientist as Science Adviser in the Office of the Under Secretary of State. With a deputy, three scientists representing the physical, life, and engineering sciences, and with liaison officers from interested government agencies on his staff, his function would be to inject scientific considerations into the policy councils of the Department. A further recommendation is the establishment of Science Staffs at selected U.S. diplomatic missions abroad. Their functions would involve collection and reporting of information on scientific progress, assistance to foreign scientists in securing American scientific publications, and facilitating two-way exchanges of scientific personnel. The report points out that "the greatest benefit in this field will emerge if the Department of State encourages and facilitates the conduct of privately sponsored programs of exchange of scientific material and persons." It concludes that "it is to the advantage of the United States to adhere to a foreign policy that will permit the fullest possible degree of personal contact among scientists."

The successful operation of the London office of OSRD, and later of the Office of Naval Research, leads one to look hopefully toward the usefulness of the proposed scientific representation in our diplomatic missions. It could be very important to have a scientist in the policy councils of the State Department who would act in awareness of the finding of this report that "there is ample evidence that unnecessary restrictions exist on the flow of unclassified scientific and technological information. These restrictions are dangerous to the progress of United States science, affect adversely the conduct of foreign relations in science, and are therefore damaging to our national security." Let's hope for action on the Berkner report.

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THE CONCEPT OF NUMBER

The Foundations of Arithmetic. Gottlob Frege. (Trans. by J. L. Austin.) xxiii + 119 pp. $4.75. Philosophical Library, New York. 1950.

THIS book gives, side by side, the German text and an English translation of a work that was published in 1884. The subtitle, "A Logico-mathematical Enquiry into the Concept of Number," indicates very well the nature of the work. The first three quarters of the book are devoted to a critical analysis of the ideas of previous writers (Kant, Leibnitz, Grassmann, Mill, Lipschitz, Hankel, Jevons, Cantor, Schröder, Hobbes, Hume, and others) on the subject of number, and the author does not find the ideas of any of these philosophers and mathematicians entirely satisfactory. His conclusion is "that a statement of number contains an assertion about a concept," and his definition of number is: The number which belongs to the concept *F* is the extension of the concept "equal to the concept *F*." Your reviewer believes that definition consists of "explanation in terms of something simpler," and simple honesty forces him to confess that his ignorance of philosophy prevents him from appreciating, or even understanding, this definition. Frege regards the number zero as belonging to the "natural" or "counting" numbers, whereas we subscribe to the view that zero is not a counting number at all (the first of the counting numbers being 1) and is only properly used when we regard number as a "relative-magnitude," zero being the relative-magnitude number which measures the relative-magnitude of two equal counting numbers.

This work of Frege's has considerable historical interest as a forerunner of the work of Whitehead and Russell. The translation is excellent and the printing leaves nothing to be desired.

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THE FLESHY FUNGI IN NATURAL COLOR

Mushrooms in their Natural Habitats. Alexander H. Smith. xiv + 626 pp. Illus. $26.50. Sawyer's, Inc., Portland, Oregon, 1949.

SOMETHING new in mushroom books has appeared in two attractively bound volumes. The work includes a Viewmaster, 231 stereophotographs in color of American fleshy fungi, a complete description of each, and discussions appropriate to the ambitious scope of the publication.

To the average student of fungi, the possibility of producing a book of value alike to the scientist, to the interested layman with no scientific background, and to the mycophagist who prefers his mushrooms cooked, would appear to be an author's dream, too tenuous for accomplishment.

With the completed project before us, however, it is obvious that Dr. Smith, foremost authority on the fleshy fungi of this country, has succeeded admirably. For the mycologist he furnishes technical descriptions employing Ridgway's colors, together with the first presentation to the public in this country of a treatment of the fleshy fungi based upon modern scientific trends. All specimens illustrated and described, with a few cited exceptions, are filed in the University of Michigan Herbarium and are available for future reference. Several new species are described, and location of their types is recorded.

For the general student and for the layman interested only in distinguishing edible from poisonous forms while collecting mushrooms for the table, popularized but accurate descriptions afford a pleasant means of learn-

ing representative forms found in various regions of the United States. An attempt was made not only to present a wide geographical coverage, but also to include as many genera and their representative species as possible.

From diagnostic keys, synonymy, and discussion of such subjects as fruiting habits of the fleshy fungi, their relation to other living organisms, how to collect and prepare specimens, microscopic characters, how fungi are named, edible and poisonous forms in each natural region of the country, and even recipes for the cooking of mushrooms, any reader can select the material best suited to his personal interests. Crowning this achievement is the collection of 231 incomparable stereophotographs in color—the first, to our knowledge, presented in such a work as this. That technical difficulties were largely overcome in their preparation is attested by the remarkable integrity of the colors, in spite of great numbers of copies made from the master films. Any slight deviations are mentioned in the descriptions.

The author, the publisher, the photographer, Wm. B. Gruber, and their colleagues are to be congratulated on an outstanding contribution to the literature of fungi. The only regret is that S. M. Zeller, late mycologist at Oregon State College, credited with initiation of the project, did not live to see the finished work.

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DEVELOPMENT OF A RESEARCH INTEREST

The Principles of Scientific Research. Paul Freedman. xi+222 pp. $3.25. Public Affairs Press, Washington, D. C. 1950.

WHATEVER the nature or extent of their formal scientific education, students find that a knowledge of science alone does not furnish the key for opening the door to undiscovered knowledge. The object of the writer has been to help in passing over the groping process in the stage commonly known as "learning by experience."

About one third of the book is given over to orientation, which seems to have more value for the individual experienced in research than for the novice. The first chapter deals chiefly with definitions and the nature and history of research. A sharp line is drawn between knowledge and discovery. In the second chapter (Research and Society), development of attitudes and general methods of procedure are treated historically, with attention chiefly to the physical sciences. The importance of sound research and the influence of society upon scientists are given especial consideration. The third chapter traces the rapid rise of science in the nineteenth century. Interdependence of discoveries in widely separated fields is used to show how fortunate accidental discoveries may yield ever-expanding results.

Without qualification and without admission that it might be a personal opinion, the author pronounces the quantum theory and the work of Einstein the greatest achievements of the twentieth century. This empirical statement seems to rest upon a supreme self-confidence in ability to pronounce definitive evaluation of incommensurates in the highly diversified fields of science.

The Mental Approach presents an analysis of qualities essential for success in research, and in nice clarity explains how observations in many fields must be made at or near the limits of observational powers. The history of most observational sciences contains many instances of unfortunate clashes between authorities because of this fact. The distinctions between textbook and original research statements are clearly explained, and the necessity of early familiarity with original papers is emphasized. Throughout the book, the importance of creative imagination and the balance between credulity and skepticism are well treated, as are also the qualities of patience, intellectual honesty, love of discovery, thoroughness, and ability to finalize.

Two chapters on planning of research have pertinent suggestions directed almost exclusively to procedures in the physical sciences.

Not only the novice about to embark on a career of research, but the seasoned investigator, the administrator, and the patron will find much thought-provoking material in this book.

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REHABILITATION TECHNIQUES

The Emotional Life of the Ill and Injured. Arthur Jess Wilson. 416 pp. $4.75. Social Sciences Publishers, New York. 1950.

IN this book, the author, who is a specialist in vocational guidance and rehabilitation, attempts to use the general principles and concepts of psychosomatic medicine in the fields of guidance, counseling, personnel relations, and rehabilitation.

The book is divided into three major parts: Part I, Solving Personality Problems; Part II, Combating Emotional Reactions to Injury and Disease; and Part III, Applying War Experience to Emotional Problems of Civilian Rehabilitation. The format is attractive, the print easily read, and the content presented in simple, clear, though rather repetitive, style.

Presentation of extensive clinical material suffers somewhat by an apparent attempt to oversimplify for the lay reader. This may be excusable because the book will be most useful to the ancillary groups related to psychiatry rather than to psychiatrists themselves.

The author is to be commended for presenting one of the first books of its kind in the area of rehabilitation where so much is yet to be learned, even though he adds no particular new knowledge or techniques. It is helpful to all to have known facts summarized and presented in an attractive way by experienced clinicians.

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A SCIENTIFIC CIVILIZATION?

Concerning Science. F. Sherwood Taylor. xii + 141 pp. $1.00. Macdonald, London; Macmillan, New York. 1949.

THIS small volume invites comparison not only with the author's earlier books in the history of science—especially *Science Past and Present*—but also with the efforts other writers have made to explain to nonscientists the nature, spirit, and methods of those sciences which are so cataclysmically changing their world. In particular, J. Arthur Thomson dealt with the same subject in his admirable *Introduction to Science,* written many years ago for the Home University Library, with such lucidity and in such memorable style that in contrast the parallel chapters by Taylor—the first three—seem pedestrian. The author does better when he comes to the industrial applications of science. Here his classification provides a good bird's-eye view of the ramifications of science in industry, which, he concludes, will lead to:

(1) Increasing the variety of products and services resulting from industry;
(2) Increasing the quantity produced per man-hour;
(3) Improving the conditions of labour by making machines to do heavy or distasteful work.

From these will flow the increase of wealth in the true sense, and the comfort of the worker both at work and at leisure.

It is the last two chapters that reveal the essential character of the author's thinking. The chapter on Science and Modern Civilisation is, in a sense, an abstract of the author's philosophy, so readily detectable in his treatment of *Science Past and Present.* The question whether civilization depends on science receives a positive answer, if by civilization we mean the highly developed urban, industrial civilization of modern Europe and America. Sherwood Taylor is well aware that this civilization is neither world-wide nor stable. Perhaps the very seeds of its own destruction are contained in every scientific civilization, namely, the "ever-increasing material interdependence of every man and organization; a lowering of standards of human virtue; and an increase of powers of destruction." The question then follows: Should we seek a scientific civilization?

In the chapter Science and the Modern Outlook, the author has made a very stimulating synopsis of the common world view of 1949, together with the dates at which the several ideas included in that world view became scientifically accepted. This is an educational masterpiece worth hours of one's study. The remainder of the chapter consists of the progressive development of a deistic philosophy through the consideration of science and the order of nature, the place of the earth in the universe and man's place in nature, the mind-body dilemma, and the scope and limitations of the scientific explanation of the world. The conclusions will not satisfy everyone, either religious or antireligious. The author, at any rate, has courageously faced a task that no thinker may shirk even though no philosopher has ever won through. If Sherwood Taylor has failed to achieve final knowledge in this quest, he has laid hold on an equal—and perhaps a far better—reward, the possession of wisdom.

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THE RED PLANET

The Planet Mars. Gerard de Vaucouleurs. (Trans. by Patrick A. Moore.) 87 pp. Illus. $2.00. Faber & Faber, London; Macmillan, New York. 1950.

THIS little book will find a ready welcome among amateur astronomers and others interested in the planet Mars. The readable and fluent style must be due in large measure to the close collaboration between author and translator. The presentation is elementary and presupposes very little previous knowledge of the subject, which is a distinct advantage for the lay reader. At the same time, this materially reduces the value of the book for the professional astronomer, since many topics are necessarily discussed in a sketchy manner.

The chapter headings indicate the nature of the material covered: Generalities, The Polar Caps, The Bright Regions of Mars, The Atmosphere, Climates, The Dark Regions of Mars, The Canals, and Life on Mars?

The illustrations are relevant and fairly good, for the most part. The four drawings on Plate III, however, which are described as "After photographs by E. C. Slipher at the Lowell Observatory, Arizona," are very poorly executed. They show much less than good reproductions of the photographs would show, and it is very well known that no reproduction can show all that is on the original negative. Illustrations such as these are apt to be very misleading.

The discussion of the controversial subject of the "canals" more or less follows the historical development of the subject, beginning with the work of Schiaparelli in 1877 and continuing with the work of Lowell and his co-workers. The observations and opinions of Antoniadi, Trumpler, Pettit, Fournier, Lyot, Gentili, Dollfus, and others are also treated. The author seems to believe that, in general, Americans are "for" the canals and Europeans are "against" them. A trace of the bitter controversies of the beginning of the century creeps in when comment is made about "the fairy tales told by Lowell." The remainder of the discussion seems to be as objective as one could expect, and the chapter ends with the statement: "Finally, we must impartially conclude that the question remains open, and calls for the most energetic research." A footnote to this statement calls attention to observations made in 1948 by A. Dollfus, working with a power of 900 on the 60-cm refractor at the Pic du Midi. During a night of perfect seeing, Dollfus was able to resolve some of the canals into minute spots and patches of irregular outline. This is certainly something that needs to be confirmed by other observers before it can be accepted without reservation. (Perhaps the only way to settle the question will

be to travel to Mars just as soon as round-trip tickets on interplanetary rockets are available!) The remarks about life on Mars are conservative, and well put.

The final conclusion seems to be that little progress in the study of Mars can be made until more adequate methods of research are developed.

The author and the translator are to be congratulated on their enterprise in bringing out this interesting book, which is the first about Mars in many years to appear in English.

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DEFINING A NEW SCIENCE

Pocket Encyclopedia of Atomic Energy. Frank Gaynor. 204 pp. Illus. $7.50. Philosophical Library, New York. 1950.

THE preface of this book indicates that its purpose is to "present a comprehensive collection of brief definitions and explanations of the terms and expressions of this new field of science to the layman with a good, average education, and to the student with a fair working knowledge of physics and at least a nodding acquaintance with higher mathematics."

There are a number of evidences, from the various entries, of the book's British origin, and it is a bit difficult to criticize the definitions and descriptions on the basis of American usage. As a result, it might well be that this book could be used by the English layman, but it seems doubtful whether it could be used by his American counterpart. For example, it is my feeling that some technical training is required before a student becomes conversant with negative powers of ten as a means of representation of decimal fractions. The *Encyclopedia* seems to assume a knowledge of this and, unfortunately, requires considerable additional technical background in order to utilize most of the entries. Of the sixteen items on the first page of the text, four or five might be understood by readers with a semitechnical background. One of these, *ampere,* has quite a technical definition, followed by: "1 abampere = 10 absolute amperes."

The *Encyclopedia* does have a tremendous quantity of material in it, however, and would probably be quite useful for "the student with a fair working knowledge of physics." This might also apply to those scientists who do not remember the details and constants of some of their early courses in physics and who would like to refer to a volume such as this to refresh their memories, and to see the methods of application to the present nuclear science of some of their early training. It seems to me that on the whole, however, the book is too technical for a novice in the field. Many of the definitions are of the handbook variety in that they merely give conventional definitions without explanation. Many of the other entries have good descriptions in readable language. For example, the author's discussion of *tracers* requires about a page of text and is quite informative. As a matter of fact, the descriptions of new terms and those that appear in the everyday press are treated rather well, for the most part. The background physics terms require the most technical background.

In reading through the entries in the book, I found some twenty-five or thirty that would probably be considered questionable—at least, in American usage. Some of these could easily have been checked, such as the fact that A. H. Compton has been away from Chicago since 1945. Other entries have some surprising conclusions, such as the one (under *isotope*) that the chemist has 96 elements, whereas the physicist employs over 300! It is a bit of a shock for the American reader to find under *sodium* that it has "no stable isotopes." This is, of course, correct under the original definition of isotope, but we are quite used to referring to such elements as being "monoisotopic."

It is also unfortunate that many of the definitions are rather loosely worded, both from a grammatical as well as from a scientific standpoint. For example, under *positron emission,* the statement is included that "positrons are emitted by radioactive nuclei which contain one or more protons too many." Other cases include repeated use of the term "one curie of beta or gamma radiation" and the use of roentgen units to mean intensities, quantities, or some other radiation characteristic—as in "1 e.r. represents the beta radiation which releases as much energy as 1 r of gamma rays in 1 g of air . . . ." The inclusion of thumbnail sketches of many workers in the field is excellent, as is the inclusion of a description of the various laboratories.

The American reader, at least, would probably compare this volume with the *Glossary of Terms* being prepared by the National Research Council (U.S.A.), excerpts from which have been published. Although the *Encyclopedia* will probably be useful as a handy reference, it is probable that the *Glossary* will be somewhat more valuable, at least from the standpoint of American usage.

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PSYCHOPATHOLOGY

Psychosexual Development in Health and Disease. Paul H. Hoch and Joseph Zubin, Eds. vii + 283 pp. $4.50. 500 pp. Illus. $7.00. Reinhold, New York. 1949.

THIS book consists of a number of scientific papers presented at the 1948 meeting of the American Psychopathological Association. Stimulated by the general interest evoked by the publication of Kinsey's data on the sexual behavior of the American male, several sociologists, anthropologists, and psychiatrists presented their observations and theories on such topics as sexual aberrations, sexual behavior of animals, childhood sexuality, and sexual patterns in various cultures. The volume is divided into five parts. In the orientation section Kinsey and associates make their well-known "plea for tolerance" toward so-called sexual deviations.

Cross-species and cross-cultural surveys, as well as animal observations, point up the fact that the Western human animal is not alone in being resourceful in his sexual activities. Papers by Henry, Hallowell, and Meade deal with problems of childhood sexuality and psychologic weaning in certain primitive cultures. Henry's views brought about a discussion in which the speakers took issue with his expressed views on the libido theory and that of sublimation.

The section on clinical and psychoanalytical studies provoked a well-organized critical evaluation by one of the editors (Hoch). It consists of an exceptionally clear presentation of some pitfalls in Kinsey's conclusions on "normalcy" and Rado's concept of sexual deviations based on castration fear.

In a comment on "therapeutic attitudes to psychosexual problems," Knight deplores the allegedly frequent practice of employing "strong-arm methods" in dealing with such problems. He uses this opportunity for a frontal attack on such treatment procedures as electroshock and lobotomy, even though they have little to do with the subject matter.

In the concluding section the sociologic approach is presented in three papers. They emphasize that the sexual behavior of an individual is determined by his experiences within a culture. The social heritage is considered to be as important (or perhaps more so) in establishing patterns as are constitution and mental organization.

Certain portions of the book make interesting reading, especially the discussions. The lack of continuity is deplorable but is hardly surprising in such a presentation. Strong prejudices for and against certain theories of sexual behavior seem to be more apparent than ever, and it is astonishing to learn how little agreement there is among authorities on such matters as normalcy, aberrations, libido theory, sublimation, and others.

The reader of this volume will find some new data and thoughtful theories but would easily become discouraged if he were to use it for reference on "psychosexual development in health and disease."

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SOIL SCIENCE

Soils: Their Physics and Chemistry. A. N. Puri. xv + 500 pp. Illus. $7.00. Reinhold, New York. 1949.

CONSIDERATION has been given to soils from three principal points of view. Part I views the soil as an acid (colloidal acid, or "acidoid"), with emphasis on the titration curves of soil acids (inorganic and humus) with various bases. The author's thesis is that colloids differ from true molecular solutions only in a reduction of active mass to that on the surface. The active mass of the soil is the mass of surface molecules. Interactions of ammonia, carbonates, carbon dioxide, sulfides, calcium, and sodium with soil acid illustrate the true acid character of soil colloids. Exchange acidity, measurement of soil pH, heat of neutralization, specific surface, ignition loss, electrolydialysis and oxidation of soils, including that of nitrites and oxalates in soils, are treated. Quality of irrigation water, soil permeability, and dispersion of sodium clays are explained.

Part II treats the particle-size distribution (termed "mechanical analysis") of soils, with emphasis on specific surface as calculated from average diameters of fractions. Various methods of particle size measurements are outlined, with considerable emphasis on apparatus ("ultra-mechanical analysis," to 4×10^{-6} cm, or 0.04 μ) particles worked out by the author and his associates. The process of dispersion, particle migration velocity, floc volume, and relation of size to specific surface and soil physical characteristics are discussed. Relationships for diameter and specific surface are not correlated in any way with modern concepts of layer silicate clays or direct measurements of total specific surface.

Part III treats soil moisture, on the basis of "capillary condensation" as a function of pore size. The moisture sorption per unit weight of solid is plotted against relative humidity of an atmosphere, or tension ("pressure deficiency") controlled by the porous plate tensiometer ("capillarimeter"). The shape of the curve relates to the micropores of the solid. Moisture sorption is considered for soils, soil fractions, salts such as $CuSO_4$, metallic oxides prepared from various salts, silica, and silicates. Water of hydration of salts is viewed as an admittance of water molecules in the micropores of the space lattice. In each case the moisture sorption curve is related to size of pores, being for metallic oxides a function of the size of radical removed from the salt used in its preparations, thus supporting the generalization for capillary condensation as the principle governing all moisture relations of solids, including soils. The author develops the thesis that in the whole range of soil moisture the only variation is the size of the pores in which it is held. No treatment is given to moisture movement in soils, or availability of soil water to plants.

Few mechanical errors are noted beyond the fact that deciphering of the legends for the graphs is frequently troublesome.

Profuse data are presented from the author and his co-workers, to support his various lines of argument. An important weakness of the book, however, is its almost total lack of documentation in the published literature; a few references to the author's own work are given, but only a small fraction of his papers is cited. One of many examples of lack of proper documentation occurs on pages 426–7; two pages are devoted to a quotation from Haines, with no reference to its source. This lack of references gives a dogmatic flavor to the work, and will hinder its full utilization.

In striving for generalization, the author has also achieved a considerable degree of empiricism. Thus, on the chemistry of soils, no consideration is given to the crystal chemistry of soil clays, or origin of exchange charges of different strengths, and the statement is made (p. 3) that "the nature of the electronegative part can be completely disregarded." The equivalent action in a

general chemistry book would be to lump all acids, organic and inorganic, as proton donors, with no further consideration of their nature.

As another example, relative to silicates and humates of soil, it is stated (p. 225): "The differences, if any, are merely qualitative, so that for most purposes, we can treat humus as an integral part of the soil. Quantitatively, of course, humus is much more reactive. . . ." This can hardly be considered a satisfactory expression of the facts of this phase of soil chemistry.

According to the preface, the book "has been written for beginners," but this purpose has not been wholly fulfilled, not only because of its empirical flavor and dogmatic lack of references, but, more important, because of omission, in many areas, of fundamental principles without which the beginning student can hardly acquire a sound concept of "the soil: its physics and chemistry." Despite these weaknesses, the author is to be commended for the preparation of this advance in the literature of soil science, which will be received with interest by soil scientists and find its place as a valuable reference book for students in intermediate college courses.

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ANTHROPOLOGY AND LINGUISTICS

Selected Writings of Edward Sapir. David G. Mandelbaum, Ed. xv + 617 pp. $6.50. University of California Press, Berkeley and Los Angeles. 1949.

Edward Sapir (1884–1939) was a remarkably gifted human being and a unique figure in American anthropology. At the time of his death, he was Sterling Professor in Anthropology and Linguistics at Yale University. He had served as president of both the American Anthropological Association and the Linguistic Society of America and was a member of the National Academy of Sciences. Besides the scholarly activities behind these honors, Sapir had deeply rooted aesthetic interests that were always a vital part of his life. He published a considerable amount of poetry, and it was he who reviewed for *Poetry* the unorthodox poems of Gerard Manly Hopkins in 1921. At one time, also, he studied composition under Edward MacDowell and occasionally published musical as well as literary criticism, examples of which are to be found in this volume. Sapir's brilliant attainments in linguistics and, in particular, the basically configurational approach to language that he adopted, are so well known that no comment is necessary here. Thus it is fitting that about half the reprinted papers fall into this category. Among them are several that transcend the minutiae of technical linguistics, dealing as they do with fundamental aspects of language as an integral phenomenon of human culture and experience. In cultural anthropology Sapir's famous methodological essay ("Time Perspective in Aboriginal American Culture," 1916), long out of print, is made available. As the editor says, this essay has assumed a classical status, being so frequently cited "in discussions of ethnological theory and closely studied by several academic generations of graduate students." The last section of the volume comprises a number of papers under the caption "The Interplay of Culture and Personality." Once considered somewhat radical in cultural anthropology, Sapir gave the most vital impetus to investigations in this area in programmatic articles, his teaching, and in other ways. He once remarked to me that he was convinced that personality and culture studies would prove to be the "spearhead" of a new anthropology.

These writings of Edward Sapir are by no means dated, although republished a decade after his death. Anthropology students may read them for the first time, and other anthropologists will want to reread them, but they should have a much wider appeal. For they are singularly fresh. They have an intellectually evocative quality that was Sapir's own. He was not only erudite and had the most subtle of minds, he was a master of the incandescent phrase; consequently, his writings still illuminate more than the present horizon of our thinking.

Thanks are due the editor for the care with which he has made his selections and the helpful introductory remarks with which each of the several sections is prefaced. It is gratifying, too, that we now have a definitive bibliography of all Sapir's published work, including his poetry.

A. Irving Hallowell

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THE PROBLEM OF STERILITY

Human Fertility and Problems of the Male. Edmond J. Farris. xvi + 211 pp. + 10 plates. Author's Press, White Plains, New York. 1950.

Attempts to transform barrenness into a state of fertility have occupied the attention of man since recorded time. Religion, magic, and rational medicine have all been employed.

Most cultures have assumed that the woman was solely at fault. In old Jewish law a man could divorce his wife if she failed to bear him a child. A wife could not divorce a husband on the same grounds. Yet in each generation there were skeptics sufficiently iconoclastic to view the male with suspicion. Aristotle reports a semen test to establish male fertility; if the semen floated on water the man was sterile, if it sank he was fertile. Cardinal Richelieu impiously advised his queenly niece, Catharine, that "a girl of spirit will never lack children."

Many articles and books have been written within the past fifty years about the problem of sterility. Most have been uniformly vapid and hugely repetitive; physicians have cloaked their ignorance by merely quoting the views and opinions of previous authorities. *Human Fertility* is a refreshing exception. In two hundred pages the author cites only thirty references. This book is Farris from cover to cover; it possesses his dynamic

drive and his clear focus. The book is unusual in another way: It is written to interest and instruct both the intelligent layman and the physician whose special domain is infertility. Amazingly enough, it accomplishes these twin purposes.

The book opens by stating the problem: "There are more than five million couples in the United States with potential fertility problems." Dr. Farris then apportions the blame: "When some abnormal condition does exist, laboratory research reveals it to be in the husband about two-thirds of the time. . . . Only about 40% of the men in this country are capable of fertilization most of the time during the peak of their reproductive lives. . . . About 20–25% will probably never become fathers. . . . Another 35–40% may never become fathers; without the best advice they will certainly as a group have fewer children than they want." If true, it is a very somber picture; it makes the reviewer ponder on the miracle that three and a half million children are born each year in this country, not to mention the siring of half a million others.

In his analysis of the male's fertility, Dr. Farris uses the unorthodox criterion of the total number of motile spermatozoa in the fresh ejaculate rather than the total count—that is, motile plus nonmotile spermatozoa. The specimen is collected after five days of sexual abstinence, and if the population of moving spermatozoa is in excess of 185 million the male can be exonerated of any blame in the sterile mating and is classified as highly fertile. A relatively fertile male possesses between 80 and 185 million moving spermatozoa. He is the man who is at least partly at fault, and in whom, after a period of abstinence, the exact timing of intercourse so that it coincides with the wife's discharge of an egg may readily cure the sterility problem. Only 3 per cent of the men with less than 80 million moving spermatozoa in their total ejaculate will ordinarily accomplish impregnation.

Dr. Farris theorizes on why it requires so many millions of spermatozoa to cause a pregnancy, when only one is actually necessary. His explanation is the "random walk" theory. He plotted the surface area of the whole female reproductive tract: vagina, cervix, uterus, and tubes, and, since spermatozoa have no intelligence with which to locate the egg, he calculated "that 70 million active spermatozoa deposited in the vagina would be required for one spermatozoon to reach the ovum in the upper third of the oviduct."

The author also describes the "Farris rat test" for determining the precise time of ovulation. Specimens of the woman's urine are injected daily for five to ten days each month into three-week-old female rats, which are killed and autopsied two hours later. By comparing the color of the ovaries of the test animals with a series of color standards, the trained observer can prophesy accurately when ovulation will occur in most women. It appears to be an excellent but impractical test, as an inexhaustible supply of rats is essential. Farris possesses this asset, however, since he is the director of the Wistar Institute of Philadelphia, which has almost as many rats as there are Philadelphians.

Dr. Farris states that at the time of publication there was no known method of materially increasing the motile sperm count of the deficient male. Drugs, vitamins, amino acids, and hormones each have their protagonists, but in his opinion each is valueless. This agrees completely with the reviewer's experience. The book states that the only hope of aiding the subfertile male is the proper timing of intercourse, and in some cases artificial insemination.

Space does not permit further discussion of this excellent, provocative, and sometimes irritating book—irritating because it is written in such a didactic and self-confident vein. Nevertheless, it is "must reading" for any intelligent layman or physician who has either a scientific or a personal interest in the problem of infertility. The reviewer cannot close without congratulating the Author's Press for the technical job; if subsequent books are printed as well as this, the first, the organization should have a fine future.

ALAN F. GUTTMACHER, M.D

Baltimore, Maryland

LAND OF THE FREEZE AND HOME OF THE BLIZZARD

*Antarctic Conquest.** Finn Ronne. 299 pp. + illus. $5.00. Putnam's, New York. 1949.

A BOOK that every geographer and man of science interested in polar or cold-temperature research should have on his bookshelf is this latest first-hand description of what it is like to get together a sizable expedition and use it as a fine tool to extract scientific facts from a land perpetually covered with ice and snow.

It is one thing to visit such a region and stay alive and well. There will be adventures aplenty to write about to make interesting reading. But when a definite program of work is laid out and the expedition comes back with physical data, geological specimens, aerial photographs, and the good topographic control for mapping that it set out to gain, then its leaders and personnel alike deserve a pat on the back.

The pat has been ably administered by the late Isaiah Bowman in the foreword. He states that real pre-eminence in any field of endeavor can be bought only at a price few are willing to pay. "We judge expeditionary leadership in terms of scientific results," he says. "With but $50,000 and government equipment, the Ronne Research Expedition was able to return with a harvest of scientific findings that would be a credit to a far more costly expedition."

There were adventures, too—particularly for Edith (Jackie) Ronne, wife of the leader, who finally stayed with the group after having served as logistics expert and recorder up to and beyond the time of sailing. She thus became the first woman to set up housekeeping in Antarctica (in a tiny 12 by 12 shack). There was the recovery of Peterson without permanent injury after

* See "Antarctic Mapping and Aerial Photography" by Commander Ronne, on page 287, this issue.

he fell and was imprisoned in a crevasse head down, 110 feet below the surface.

Another high light of the expedition was the rescue of three British airmen who had lost their way in bad weather and crash-landed on sea ice in Marguerite Bay forty miles from base camp. There were also international complications through conflicting claims by Britain and Chile over the area in which the expedition worked. The United States has never made any official claims to land in Antarctica, even though the entire peninsula is labeled by American geographers "Palmerland," after Nate Palmer of Stonington, Connecticut, skipper of the sloop *Hero*. He was the first to visit this area in 1820.

But not until the Ronne Expedition did its thorough job of exploring and mapping newly discovered areas between the Bellingshausen and Weddell Seas was it definitely established that Palmerland is a peninsula, not an island. This new land at the base of the peninsula is now named Edith Ronne Land and covers several hundred thousand square miles of antarctic territory, which the expedition mapped by aerial photography and tied in astronomically with transit measurements taken by ground crews.

Even if the United States never lays formal claim to any antarctic land, the geographic work accomplished by these explorers will leave its mark for all time on maps of this portion of the globe. Those who helped make the expedition a success will likewise be remembered through the Sweeney Mountains, Lassiter Shelf Ice, Gould Bay, Lowell Thomas Mountains, Wright Inlet, Tufts Valley, Nichols Glacier, Hilton Bay, and Mounts Peterson and Tuve.

HERBERT B. NICHOLS

U. S. Geological Survey
Washington, D. C.

New Books Received

Beginning Experimental Psychology. S. Howard Bartley. viii + 483 pp. Illus. $4.00. McGraw-Hill. New York. 1950.

The Australian Environment. (2nd ed.) 183 pp. Illus. 10*s*.6*d*. Commonwealth Scientific and Industrial Research Organization. Melbourne. 1950.

The Early Development of the Concepts of Temperature and Heat. James Bryant Conant, Ed. iv + 106 pp. Illus. $1.25. Harvard University Press. Cambridge. 1950.

The Cave Book. Charles E. Hendrix. 68 pp. Illus. $1.00. Earth Science Publ. Revere, Mass. 1950.

The Natural Philosophy of Plant Form. Agnes Arber. xiv + 247 pp. Illus. $5.00. Cambridge University Press. New York. 1950.

Some Theory of Sampling. William Edwards Deming. xvii + 602 pp. Illus. $9.00. Wiley. New York. 1950.

Blindness. Paul A. Zahl, Ed. xvi + 576 pp. Illus. $7.50. Princeton University Press. Princeton, N.J. 1950.

Saints, Sinners and Psychiatry. Camilla M. Anderston. xiv + 206 pp. $2.95. Lippincott. Philadelphia. 1950.

Measurement and Prediction. Stouffer, Guttman, Suchman, Lazarsfeld, Star, and Clausen. x + 756 pp. Illus. $10.00. Princeton University Press. Princeton, N.J. 1950.

An Introduction to Plant Physiology. Otis F. Curtis and Daniel G. Clark. xiii + 752 pp. Illus. $6.50. McGraw-Hill. New York. 1950.

The Effects of Atomic Weapons. x + 455 pp. Illus. $1.25. Los Alamos Scientific Laboratory. Los Alamos, N.M. 1950.

A Laboratory Manual in Animal Parasitology. H. W. Manter. ix + 121 pp. Illus. $2.25. Burgess Publ. Minneapolis. 1950.

Life Histories of North American Wagtails, Shrikes, Vireos, and their Allies. Arthur Cleveland Bent. vii + 411 pp. Illus. $1.50. Smithsonian Institution. Washington, D.C. 1950.

Qualitative Analysis. (Pt. 3.) *Analytical Procedures.* J. C. Hackney. viii + 139 pp. Illus. $1.75. Indiana University. East Chicago, Ind.

German-English Dictionary for Chemists. (3rd ed.) Austin M. Patterson. xviii + 541 pp. $5.00. Wiley. New York. 1950.

Gray's Manual of Botany. (8th ed.) M. L. Fernald. lxiv + 1632 pp. Illus. American Book Co. New York. 1950.

Saturating Core Devices. Leonard R. Crow. xii + 373 pp. Illus. $4.20. Scientific Book Publ. Vincennes, Ind. 1949.

Learning Electricity and Electronics Experimentally. Leonard R. Crow. xi + 525 pp. Illus. $4.40. Scientific Book Publ. Vincennes, Ind. 1949.

Organic Chemistry. (2nd ed.) Louis F. Fieser and Mary Fieser. xv + 1,125 pp. Illus. $7.50. Heath. Boston. 1950.

Man the Maker. R. J. Forbes. 355 pp. Illus. $4.00. Schuman. New York. 1950.

The Rise of Words and their Meanings. Samuel Reiss. 301 pp. $3.75. Philosophical Library. New York. 1950.

Bird's-Eye View of the Pueblos. Stanley A. Stubbs. xviii + 122 pp. Illus. $3.00. University of Oklahoma Press. Norman. 1950.

Hypnodrama and Psychodrama. J. L. Moreno and James M. Enneis. 56 pp. $2.75. Beacon House. New York. 1950.

Bertrand Russell. H. W. Leggett. 79 pp. Illus. $3.75. Philosophical Library. New York. 1950.

Antimetabolites. D. W. Woolley, Ed. 184 pp. Illus. $2.75. New York Academy of Sciences. New York. 1950.

Verständliche Element der Wellenmechanik. Karl Jellinek. xii + 304 pp. Illus. Swiss Fr. 34.— Wepf & Co. Basel, Switzerland. 1950.

Weltsystem, Weltäther und die Relativitätstheorie. Karl Jellinek. xv + 450 pp. Illus. Swiss Fr. 45.—Wepf & Co. Basel, Switzerland. 1949.

Colloid Science. James W. McBain. 450 pp. Illus. Text edition, $6.00; trade edition, $8.00. Heath. Boston. 1950.

The Practical Application of Acoustic Principles. D. J. W. Cullum. 200 pp. Illus. $3.00. Macmillan. New York. 1950.

The Challenge of Cancer. Lester Grant. vii + 116 pp. Illus. 55¢. National Cancer Institute. Bethesda, Md. 1950.

[*Continued on advertising page xii*]

CORRESPONDENCE

THE SEVEN SAGES OF THE GROVE

IN THE third century, A.D., lived the Seven Sages of the Bamboo Grove, according to Taoist legend. They all "revered and exalted the void, and nonaction. . . . They drank wine to excess, and disregarded the affairs of this world, which seemed to them like duckweed."

The Seven Sages of the Grove
Delighted in nonaction.
And in ignoring things they took
Ignoble satisfaction.

With Problems or Discussion
They quite refused to cope.
They couldn't tell a Russian
From a neutral isotope.

They never smashed an atom,
They never heard of Freud;
And their desideratum
Was zero, unalloyed.

I cannot bear to think of all
The Nullity they throve on!
(Has anybody got a map
That I could find that Grove on?)

JESSIE HIRSCH

Santa Monica, California

THE ABUSE OF POWER

YOUR review of *Science and Civilization* says: "Although we can't get along without science, we don't know how to live with it. . . . Power has become a giant, but judgment of its use has remained a baby. . . . How is it that this same puny baby has been able to make such a mighty toy?"

The puny baby never made the toy. It's a mistake to say that "man" has created this power, now "man" must learn to use it. Which men? The toy was developed by rare men of superior intelligence, but the judgment is wielded by common men of average intelligence. This, then, is the problem which you say the book does not clearly define. Science is developed by people of high IQ and used by people of low IQ. Like handing a match to a moron, it makes us uneasy, for the match is a splendid tool—but not in the hands of those incapable of assessing the pros and cons for its use.

Rare men did the thinking that made the toy possible; others not so rare used this thinking to make the toy a reality, and it is handed to the baby—that is, given over to the judgment of poor, dull, mass man—by others not rare at all: popularizers, advertisers, manufacturers, educators, do-gooders—in fact, anyone who can use a profit on the toy, or who mistakenly thinks mass man, with a little help, can understand it well enough to be responsible for its use.

Since we believe in rule by majority vote, then the use of the magnificent power of science will continue to rest on the judgment of those who cannot understand it. The most pertinent example is atomic energy. The scientists who thought out fission are not the ones who judged it best used in a destructive bomb.

There are no feasible answers—science for scientists only is out of the question. And increasing the mass intelligence is also impossible, for intelligence can't be taught, like a trick, but must be inborn, like blue eyes. The most that can be done is to teach people to use what intelligence they have.

Since suppressing the printing press and practicing eugenics are not likely to come about, ordinary men will continue to direct extraordinary power, and all of us, ordinary and extraordinary alike, will continue to quake in our boots and write well-meaning books on "How is it that this . . . puny baby has been able to make such a mighty toy?" and "How may the baby be entrusted to play with the toy?"

J. H. HOLMES

Riverdale, New York

THE SEPTEMBER ISSUE

AS A seventy-four-older who was brought up on *The Popular Science Monthly,* I find your September issue very interesting. . . . I was reading the magazine in 1891 and well remember the articles by Huxley, Spencer, and especially White's "Conflict of Science and Religion." Much of this material was over the head of a young boy, but it made a lasting impression.

CARL D. GREENLEAF

Elkhart, Indiana

PERMIT me to congratulate you on the delightful Historical Issue of THE SCIENTIFIC MONTHLY. I read it from cover to cover in one afternoon and enjoyed every page of it. I think the experiment should be repeated maybe every fifth or tenth year. . . .

HUGO ILTIS

Mary Washington College of the University of Virginia
Fredericksburg

CONGRATULATIONS on the September 1950 Historical Issue. I think this was a fine idea, and it is a splendid selection, much to the credit of the AAAS.

M. A. TUVE

Carnegie Institution of Washington

. . . I HAVE been much enjoying your September 1950 issue of THE SCIENTIFIC MONTHLY. I particularly enjoyed those articles that went back well into the last century. . . .

F. W. PRESTON

Preston Laboratories
Butler, Pennsylvania